











BRAITHWAITE'S RETROSPECT.

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VOL. LXVI. JULY—DECEMBER, 1872.



THE  
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND  
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN  
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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# SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING  
AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED  
BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

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## AFFECTIONS OF THE SYSTEM GENERALLY.

FEVER AND SOME ACUTE INFLAMMATIONS.—*Stimulants*.—The following may be said to have been actually demonstrated, a fact beyond further argument: that alcohol administered in small and oft-repeated doses does not excite or increase the inflammatory process, and that inflammation may cease while a patient is taking considerable quantities of alcohol. Mild cases of fever, however, do not require alcohol. Dr. Todd himself never used it except in cases of severe inflammation or fever. There are again differences in the constitution of the patient, requiring corresponding modifications of treatment. Some persons are better treated without alcohol, while others suffering from the very same disorder require a good allowance of stimulant. (Dr. L. S. Beale, p. 12.)

*Why do we give Wine in Fever?*—Not because the tongue is brown or moist, not because there is delirium, not on any other account than because the state of the circulation requires it. Alcohol is the remedy for failure of the heart's power, and this may occur with or without delirium, and with a dry brown tongue or a moist one. (Dr. L. S. Beale, p. 19.)

GOUT.—*New Theory of*.—Gout is not a simple excess of uric acid in the blood, it is an affection of the nervous system. The predisposing cause of gout is undoubtedly the presence in the blood of uric acid, and of soda in some form. Nerve force, when in a healthy condition, preserves these in a fluid state, separately, in a condition in which they may be eliminated by the skin, kidneys, or bowels; as soon, however, as this nerve influence is lessened, these two substances unite, and the disease is produced. The action of colchicum is one proof of this, It is a nerve stimulant, producing invigoration of the mental faculties, with increased action of all the excretory organs. (Dr. Meldon, p. 48.)

**RHEUMATIC GOUT.**—*Constant Galvanic Current.*—In cases of rheumatic gout, where the most approved medicinal and hydrotherapeutic treatment, intelligently and perseveringly followed, have left the patient no better but rather worse, the constant current may often do a great deal of good. It relieves pain, and for this purpose is far better than the administration of morphia either by the mouth or subcutaneously. It should be applied to the suffering parts, so that the positive pole, armed with a small electrode, is made to touch the sore points, while the negative, connected with a large electrode, is placed in the neighbourhood. The current used may be tolerably powerful. It also has some power in removing deformities of joints actually existing. No one who has not seen a case, can have any idea of the beneficial effects which the current will produce, in the long run, on these deformities, even when they are extensive, and where the patients are not very old. A rapid cure must not, however, be expected—perseverance is required. (Dr. J. Althaus, p. 53.)

**SMALL-POX.**—*Injection of Lymph.*—The ordinary process of vaccination by scratching the arm will modify small-pox in infants, but it is almost inoperative in adults. If, however, pure vaccine lymph be injected into the subcutaneous fascia to the amount of the contents of one or two tubes, the disease although actually commencing will be arrested, or if rather further advanced will be modified considerably in its course. For example, two tubes of lymph were injected into the arm of a patient in whom the papular eruption was appearing on the hands and forearms. By the third day all eruption had disappeared. Two tubes of lymph were injected in a case on the second day of the eruption, the patient being unvaccinated and of dissipated habits. The eruption was not arrested, but it began to desiccate on the ninth day instead of the fifteenth, eighteenth, or twentieth. (Mr. R. C. Furley, p. 42.)

*Vaccination a Protection from Small-pox.*—The period of incubation of small-pox is thirteen days. Vaccination, therefore, has time to run its course and protect the system before the small-pox pustules appear. The most common result, however, is the occurrence of severe symptoms ending with the appearance of a few spots, sometimes of none. But if the symptoms of small-pox commence before the vesicles have risen and begun to desiccate, he will have a severe attack, but modified in so far that there will be no secondary fever. This plan of treating small-pox is useless unless done within a day or two of the onset of the disease. (Dr. W. Marshall, p. 38.)



**TYPHOID FEVER.**—Animal broths and jellies must be rigidly excluded from the diet. Milk should be the chief article of diet, having a little ice added to it if agreeable, and a little lime-juice in cases in which it returns curdled. Rennetted milk, rice milk, custard, rusks and hot milk, or blanc-mange, generally afford sufficiently varied ways of giving milk. Two or three cups of really good tea or coffee should be given between daybreak and two in the afternoon, every day, unless there is some evident contra-indication in the state of the nervous system. Dr. Parkes found that coffee increases the elimination of urea in fever, and there is no doubt that both it and tea lessen drowsiness and prostration. If early in the fever there is flushed face and headache, a high temperature, and thickly-coated tongue, four or six grains of calomel should be given. Ergotin administered hypodermically is the best remedy in case hemorrhage from the bowels occurs. The following is a good pill to check excessive action of the bowels: three grains of bismuth, one-sixth of a grain of opium, and one-sixth of a grain of carbolic acid. (Dr. J. Little, p. 9.)

*Diarrhœa of Typhoid Fever.*—The diarrhœa should be checked. The chalk mixture is good, but the chalk has to pass through many feet of intestine before it reaches the part desired. A better mode of treatment is to fill the lower bowel with a starch injection. (Sir W. Gull, p. 5.)

*Delirium in Typhoid Fever.*—In the treatment of this complication remember that it is owing to brain-irritation, not to inflammation. There is one great remedy for this, and it is alcohol; alcohol is the remedy for nervous irritation in typhoid fever, and, in fact, in any fever. How it acts is not certainly known, but it may be laid down that it is an important point in the treatment of all fevers. It allays nervous irritation and soothes the nervous system. (Sir W. Gull, p. 5.)

*Ulceration of the Vagina, Larynx, and other parts, in Typhoid Fever.*—There may not only be the ordinary ulceration of the ilium in cases of typhoid fever, but ulceration of other parts. A young girl was sickening of an illness. She had a discharge from the vagina, which led the medical attendant to doubt the girl's chastity; the case was really one of typhoid with ulceration of the vagina. Another case was treated for gonorrhœa; but in reality the patient had typhoid fever with ulceration of the prepuce. Another patient had ulceration of the larynx in the course of typhoid fever; he suddenly became emphysematous. (Sir W. Gull, p. 4.)

## AFFECTIONS OF THE NERVOUS SYSTEM.

**COLLAPSE.**—*Injection of Ammonia into Veins in Collapse Threatening to be Fatal.*—In cases of profound collapse, or in opium poisoning, there is no stimulant equal to ammonia injected into a vein. The solution of ammonia should consist of eight minims liq. ammoniæ fort., with two drachms of cold water. One of the superficial veins of the forearm is to be exposed, and an aneurism needle passed under it; a ligature is then placed on its distal side, while a running noose is left in position over its proximal portion. The finger of an assistant is then placed on the vessel, about a quarter of an inch above the proposed opening, for the purpose of preventing the entrance of air. A slight slit, sufficient to admit the nozzle is then made into the vein. The syringe, having been charged and all air expelled, is introduced into the vein, and a little fluid injected to dispel the air between the occluding finger of the assistant and the aperture. Simultaneously the aforesaid nooze, embracing the vessel, is to be tightened round the nozzle, thus shutting off the connection with the external opening. (Dr. Macewen, Glasgow Med. Jour., Aug. 1872, p. 493.)

**EPILEPSY.**—Hereditary or secondary epilepsy must be considered radically incurable, but induced epilepsy is often amenable to remedies. Each case must be carefully gone into in order to discover and remove the cause of the irritation. The most common causes, such as blows, loss of blood, syphilis, anæmia, &c., are well known. Epileptics are frequently the subjects of constipation, and conversely, constipation, especially in children, is one exciting cause of epilepsy. (Dr. J. T. Dickson, Lancet, Aug. 31, p. 293).

*Bromide of Potassium.*—Out of 207 cases in which this remedy was well tried, 17 were permanently cured, 28 remained free from one to two years, 33 improved considerably, 19 less so, and in 110 there was no result. The commencing dose should be half a drachm daily, and it should be gradually increased up to a drachm and a half or two drachms. (Dr. Legrand du Saulle, p. 70.)

**HICCUGH AND SPASMODIC AFFECTIONS.**—*Oil of Amber.*—Oil of amber is an antispasmodic of considerable value in cases of hiccough and other spasmodic affections. In the Pharmacopœia of 1809 it was introduced, combined with ammonia, as “Spiritus ammoniæ succinatus.” In a case of severe cramps in the limbs during pregnancy the following was the prescription used: *R. Olei succini. 3 ss.; liq. potassæ, 3 j.; tinct. camph. co. 3 ss.; aquæ meuth. pip. ad 3vj. M.: an ounce to be taken at bedtime.* (Mr. T. T. Griffith, Brit. Med. Journal, April 20, p. 422.)



**HYSTERICAL AND CEREBRAL OR SPINAL PARALYSIS.**—*Diagnosis between.*—In cerebral paralysis the nails of the palsied limb cease to grow; in hysterical paralysis they grow as usual. (Dr. S. W. Mitchell, *Lancet*, June 29, p. 911.)

**LOCAL PARALYSES.**—*Injections of Strychnia.*—The way to use strychnia subcutaneously in cases of local paralysis is, not to inject the weak pharmacopœia solution, for its action is too great in consequence of its rapid absorption, and its effects are also consequently transitory. If, however, a very small amount of a stronger solution is used, its absorption into the system is slow and its effects prolonged. Thus used strychnia has proved successful in the hands of Mr. Barwell in several cases of facial paralysis, several of paralysis of the wrist (independent of syphilis or of lead poisoning), and especially does it afford a hope of curing otherwise incurable cases of infantile paralysis. Mr. Barwell's directions about the strength of the solution and mode of using it will be found in the article. (Mr. R. Barwell, p. 393.)

**SICK-HEADACHE.**—*Guarana.*—Guarana consists of the powdered seeds of *Paullinia Sorbilis*, a tree growing in Brazil, and contains an alkaloid identical with that found in tea and coffee. It is a remedy of some value in sick-headache. A medical man in Montreal, who is subject to this complaint writes, "By taking one of these powders and remaining quiet when I have felt premonitory symptoms, . . . I have carried off the attack; and, with the first box, absolutely put it off for two months—something which had never occurred in my life before." (Dr. S. Wilks, Dr. Latham, p. 68.)

**TETANUS.**—Examine the wound carefully. A case in the practice of Dr. Ebenezer Watson, of Glasgow, was cured by the removal of a small gravel stone from a lacerated wound of the hand. Dr. Fergusson, of Peebles, relates a case in which a small spiculum of bone was found in the wound. Its removal was followed by a very marked remission in the symptoms. In this case he gave the enormous quantity of 120 grains daily of choral, and 90 grains of bromide of potassium, for four weeks. There was no bad effect on the general health as might have been expected. (Dr. A. Fergusson, p. 63.)

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## AFFECTIONS OF THE CIRCULATORY SYSTEM.

**ANEURISM.**—*Occlusion of the Efferent Artery.*—Nature herself has shown us that an aneurism may be cured by occlusion of the efferent artery. A large aneurism of the aorta co-existed with a small one of the subclavian artery, and the

aortic aneurism had compressed a portion of the subclavian artery beyond the aneurism so as to obliterate it. The aneurism of the subclavian was nearly filled with layers of coagulum. It has been proved by actual experience that an abdominal aneurism can be mechanically filled with clot, even to its occlusion, by means of mechanical pressure applied to the efferent vessel for only a few hours. The forms of aneurism in which ligature of the efferent vessel or its occlusion in some other way are likely to be called for, are certain examples of abdominal aneurism and of aneurism of the innominate and first part of the subclavian and common carotid arteries. In aneurism of the common iliac and external iliac it may also be of value. (Mr. T. Bryant, p. 177.)

*Intra-Thoracic and Abdominal Aneurisms.*—Before resorting to more strictly surgical measures of treatment, by which is especially meant, galvano-puncture, we should try what can be done by simply “internal” treatment. A case is related by Mr. Jolliffe Tufnell, in which an aneurism of the arch of the aorta became completely consolidated by this plan. The patient should be confined strictly to the recumbent posture during a very long period, the most minute precautions being taken to ensure that he can be kept without raising himself once from the bed during many weeks; his food should be restricted to the smallest quantity of nutritious solids that can satisfy his appetite; and his drink to the very smallest quantity which can be tolerated. An occasional small venesection is serviceable, but large bleedings are to be avoided. The medical treatment of the case otherwise consists almost exclusively in the administration of such narcotics as may be absolutely necessary to calm pain and procure sleep, and such purgatives as may be required to keep the patient comfortable, and avoid all straining in defecation. The great object is to keep the pulse at an equable rate—a little over 60—and free from all excitement. (Prof. T. Holmes, p. 157).

**CARDIAC DISEASE.**—Cardiac disease before forty, is more frequently on the left side of the heart than the right, and is the result of inflammatory change; after forty, disease is more frequent on the right side than the left, and is the result of tissue degeneration. (Dr. C. Black, p. 78).

Degeneration of the tissues of the body begins at forty. Degeneration of the muscular structure of the heart is especially predisposed to by the habitual respiration of impure air in closely-shut, non-ventilated, hot, stifling rooms, whether in private houses, public buildings, theatres, schools, or manufactories. (Dr. C. Black, p. 81).



## AFFECTIONS OF THE RESPIRATORY SYSTEM.

**ASTHMA.**—The following is the result of direct experience:—

The contractility of the bronchial muscles, *i. e.*, the circular muscular fibres of the tubes, is influenced differently from that of other muscles, by various poisonous or remedial agents. Thus, hydrocyanic acid does not impair it at all, opium and morphia very little, conium and aconite a little more, but belladonna and stramonium almost destroy it; so that in animals poisoned by these drugs the bronchi showed scarcely any signs of contractility when stimulated. In animals poisoned by strychnia, the bronchi seem permanently contracted, so that a stimulus has no further effect. It is worthy of remark that the action of these several drugs on the bronchial tubes is quite different from what it is on the œsophagus and intestinal canal. (Dr. C. J. B. Williams, p. 91).

**CONSUMPTION.**—*Koumiss.*—Koumiss is milk which has undergone the *vinous* fermentation. Warmth and frequent agitation are necessary during the process, or we should have the acetous instead of the vinous fermentation. The milk must be frequently agitated, and the portions which have separated be beaten up with the rest. A portion of sour milk is used as the ferment. If well secured in closed vessels and kept in a cool place, it may be kept a long time. It is highly beneficial as a dietetic, and possessed of considerable virtue as a medicine. In cases of general debility with consumptive symptoms, and in typhoid fever, it is of great use. In the former case patients may drink two quarts daily; and if it acts as in an illustrative case given, they will get fat upon it. (Mr. J. T. George, Brit. Med. Jour., Sep. 21, p. 325.).

*Contagiousness of Consumption.*—Consumption cannot be contagious as supposed by many. It is a purely constitutional disorder, which may be inherited, or acquired, but not caught. Out of all the nurses employed at the Consumptive Hospital, there has only been one case of consumption, and she was not fit for much at first. Of those now resident, nine have been resident seven years and over, two seventeen years. (Dr. R. P. Cotton, p. 111).

**CROUP.**—The majority of cases of croup have no false membrane in the trachea or bronchial tubes. The disease is essentially an inflammation of the mucous membrane of the larynx and trachea, accompanied with secretion of tenacious mucus, and swelling caused by effusion into the sub-mucous areolar tissue. The treatment is, a warm room, the air of which is thoroughly saturated with moisture by

means of a boiling kettle pouring its steam into the room, linseed poultices to the throat, and an emetic of ipecacuanha. (Dr. R. C. R. Jordan, p. 102).

*Chloral and Ipecacuanha in Croup.*—In a bad case of croup with urgent dyspnœa, give to a child of fifteen months old two minims of ipecacuanha wine, with two grains of chloral every two, three, or four hours, according to the effect produced. (Dr. J. Barclay, p. 108.)

**HOOPING COUGH.**—Dover's powder is a remedy worth trying in whooping cough. It should be given in doses of one grain thrice daily to a child twelve months old. It lessens the number and the severity of the attacks, and soothes the general irritability which so often exists. (Dr. W. Maccall, p. 109.)

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## AFFECTIONS OF THE DIGESTIVE SYSTEM.

**HEMORRHOIDS.**—Unquestionably the best method of treating bleeding internal hemorrhoids is the ligature. It is the simplest, safest, easiest, and most effectual. Nitric acid may be very well in flat sessile piles, but it is of little use where the piles are large or pedunculated. Chloroform should not be given unless in the case of a sensitive female, as you lose the voluntary assistance of the patient in forcing down the hemorrhoids. Large tumours should be tied in two halves. The ligatures should be cut off short, and, the parts being first oiled, the whole should be returned into the rectum. The ligatures separate usually about the sixth or seventh day. (Dr. Van Buren, p. 213.)

Be very careful in ligaturing a pile not to include any of the proper mucous membrane of the bowel, or great unnecessary pain will be caused. It is better not to tie the tumour too close to its base. (Dr. Bodenhamer, p. 216.)

**STRANGULATED HERNIA.**—Do not go on trying to reduce a hernia if you find it difficult to do, particularly if no previous treatment has been pursued. Especially of use is a hot bath and two hours in blankets afterwards. It is better not to attempt reduction in the bath. If, however, the hernia is not then reducible, chloroform should be given so as to reduce muscular resistance to a minimum. It is of most service in recent or recently enlarged hernia and in inguinal rather than femoral hernia. There are some cases in which reduction should not be attempted without operation, as when the patient vomits fecal matter and has peritonitis or



is in collapse, with a small rapid pulse, hiccough, or other such extreme signs. The risk of the operation is trivial in comparison with that of returning sloughs or ulcerating intestine into the abdominal cavity. There are many other valuable rules given by Sir James Paget in the article from which the above is taken, but which it is impossible to epitomise. (Sir J. Paget, p. 192.)

*Is an inflamed hernia strangulated?*—The local symptoms present in a strangulated hernia may be present in a hernia which is inflamed and not strangulated. It is rare, however, that the general symptoms—the constipation and vomiting, the quickened pulse and breathing, and the rest, exist when a hernia is inflamed but not strangulated. This is a case in which the diagnosis is very important. It can generally be made thus: in the inflamed hernia without strangulation the local signs precede, and greatly predominate over the remoter and general signs; while in a hernia which is inflamed after becoming strangulated, the remoter and general signs will still predominate over the local, and the history will tell that they preceded. (Sir J. Paget, p. 188.)

*Opening the Sac in Hernia.*—We should always operate without opening the sac if possible, especially in old and feeble people, and in large herniæ. But our wish to do this must not lead us too far. For there are many cases in which the contents of the sac are not fit to be returned into the abdomen. We should always suspect that the contents of the sac are unfit to be returned in these sets of cases: 1, in those in which strangulation has existed long, say four or more days, whether with slight or with severe symptoms; 2nd, those, of whatever date, in which the signs of strangulation are very acute; 3rd, those in which there are very marked signs of advanced or low inflammation in and about the sac; 4th, those in which the contents do not go back easily, and within two or three minutes after all stricture is fairly relieved. (Sir J. Paget, p. 197.)

*Seat of Stricture in Inguinal Hernia.*—The seat of stricture in the majority of the cases of inguinal hernia I have operated on, was formed by the mouth of the sac, thickened and hardened by what seems to have been an inflammatory process. This annular thickening may, in some cases, be thinned from the outside sufficiently to yield, but more often it is necessary to open the sac and divide the stricture from within. Remember that an easy reduction from an open sac is better than a difficult reduction from a closed one. (Sir J. Paget, p. 198.)

*After Relief of Strangulation, is the gut to be returned into the abdomen or not?*—If the fluid let out of the sac is turbid, brownish, or muddy, careful consideration is required. If it has a distinct fæcal or putrid odour the gut must not be returned. Clear fluid, merely containing flaky lymph or blood, is a favourable sign of the state of the intestine. The gut itself, for safety of return should be thin-walled, firm, tense, and elastic, preserving its cylindrical form, smooth, slippery, and glossy. You may return intestine of any colour, short of black, if its texture is good, and its surface shines and glistens; but if the black textures are lustreless, soft, flaccid, or viscid, sticking to the fingers, or looking villous, the parts should not be returned, colours about which there can be little doubt; for signs of gangrene are white, grey, and green, all dull, lustreless, in blotches, or completely over the whole protruded intestine. (Sir J. Paget, p. 202.)

*Omentum contained in a Hernial Sac.*—A small piece, not adherent, and not more changed than by congestion or slight inflammation, should be returned after the intestine. A very large piece, in the same condition, should also be returned, if it can be done without much force. If, however, the abdomen is tense, with over-filled intestine, and it cannot be returned without much force, it should be left in the sac. A small piece, hardened and thickened by old disease, may be returned, a large piece so thickened is better left. When omentum is adherent, but in other respects fit to be returned, you should break the adhesions and return it, after stopping all bleeding. If it be not fit to be returned, leave the adhesions; and in any case do not break adhesions so near the mouth of the sac that their vessels are likely to bleed into the abdominal cavity. (Sir J. Paget, p. 203.)

*Failure of Relief by Operation for Strangulated Hernia.*—There are some cases in which no relief is given by the operation, the symptoms continue just as if no operation had been done. This may arise from—1, utter exhaustion of the patient; 2, the existence of a constricting band, or the misplacement of the intestine which had been overlooked; 3, the bowel which had been strangulated remaining nipped, powerless, and incapable of contraction, while the part of the canal above it becomes distended; 4, the existence of peritonitis before the operation, which is not relieved by it but continues.—(Sir J. Paget, p. 207.)

*Aspirating Puncture of Strangulated Hernia.*—A case of strangulated inguinal hernia is related by Dr. Leon Labbé, of Paris, in which strangulation had existed about eighteen



hours. The patient was seventy years of age, but was in excellent health otherwise. All efforts at reduction having failed the No. 2 needle of the aspirator was introduced, and two drachms and a half of a yellowish liquid immediately escaped along with a quantity of gas. The tumour flattened immediately and a very gentle pressure exerted for one minute near the neck of the sac caused complete reduction of the hernia. (Dr. L. Labbé, p. 212.)

It seems to be clearly established that puncturing the intestine freely may be resorted to with every prospect of success. In one case M. Demarquay drew off 120 grammes of intestinal liquid by means of an aspirator. The credit of first discovering this plan of affording relief to strangulation belongs to Mr. Bryant, of Guy's Hospital, who punctured a large irreducible hernia, in four or five places, with a grooved needle and let out a large quantity of flatus, but no faecal matter. (Mr. Bryant, M. Demarquay, p. 211.)

**PYROSIS.**—Iodine is a remedy of unquestionable efficacy in checking that kind of vomiting which constitutes what is called pyrosis. Three to five drops of the tincture should be given in an ounce of water. Although this remedy is of value, nitrate of silver, given in pills, immediately after food is perhaps more trustworthy. (Dr. J. K. Spender, Brit. Med. Journ., June 15, p. 636.)

#### AFFECTIONS OF THE URINARY ORGANS.

**HYDROCELE.**—*New Method of Treating.*—The serous tunic of the testicle is from its nature very liable to take on adhesive action. If a hydrocele is simply tapped, and then the parts equally and firmly strapped, the sac will be obliterated and a radical cure effected. The pressure should be kept up for about three weeks, and the parts never allowed to be pendant during that time. If a case is not cured in this way, inject with iodine in small quantity, and then stick with plaster as before. This plan is free from all risk, which cannot be said of some of the other plans in use, such as the use of a seton and the passing a probe into the sac coated with some caustic. (Mr. S. M. Bradley, p. 251.)

**INCONTINENCE OF URINE IN BOYS.**—In many cases this is owing to adhesion between the prepuce and glans penis. (Mr. J. C. Forster, p. 245.)

**LITHOTOMY.**—In this operation it is not the brilliant, but the careful operator, who is the most successful. The great rule to remember is this, that the minimum of deep cutting gives

the most successful results. Having guided the point of the scalpel into the groove of the staff by the finger nail well pressed down into the groove, the passage of the scalpel forwards so as to cut into the prostatic part of the urethra should be done very carefully, and when the prostatic part is well reached, the operation should, if possible, be concluded by the finger, the forceps, and the scoop. More cutting than this should rarely be done. If it is a large stone the prostate may be divided a little more freely with the knife, and then the wound allowed to be dilated by stretching or tearing of the tissues as the stone is withdrawn through it, which must be done slowly and steadily, though considerable force may have to be used. The staff should never be withdrawn from the bladder till the finger is upon the stone. (Prof. Humphry, p. 238.)]

**SOUNDING FOR STONE.**—In sounding for stone always take a sound with a very short beak. You are much less likely to miss a stone with such a sound than you are with a longer beaked one, for this reason, that the beak can be turned downwards and so will reach a stone quite in the fundus of the bladder, or in the pouch behind an enlarged prostate. In sounding adults it is a good plan to pass a lithotrite at once, and if a stone is detected to crush it at once. This saves time. An instrument with non-fenestrated blades is the best as the fragments are more completely crushed by it. (Dr. J. C. Forster, p. 244.)

**SPASM OF THE BLADDER.**—*Continuous Electric Currents.*—If a continuous electric current is passed through a bladder firmly contracted upon a calculus, or in a state of great irritability, relaxation of the spasm is produced. The action is immediate, and no after ill effects show themselves, as when medicinal substances such as opium are employed. This effect of electricity is of the greatest importance, as it allows of examination of a stone prior to operation, the size and general configuration of which it would otherwise have been impossible to determine. (Dr. Reliquet, p. 115.)

**STRICTURE OF THE URETHRA.**—A great number of useful bougies are used on the continent for this affection which are unknown in England. The wood cuts at page 247 will at once give an idea of their nature. (Mr. W. F. Teevan, p. 246.)

**SUGAR IN URINE.**—*New Mode of Detecting.*—If much colouring matter or creatine be present in urine the suboxide of copper when found is not precipitated, and what is more important a large amount of uric acid will reduce the copper the same as



sugar does. To obviate this source of fallacy filter one or two ounces of the urine several times through good animal charcoal, until the liquid passes through nearly colourless. Then wash the charcoal on the filter with a little distilled water; and to this water, when filtered off, apply Trommer's test. (Dr. J. Seegen, Vienna, p. 120.)

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#### AFFECTIONS OF THE BONES AND JOINTS, &c.

**CARBOLIZED CATGUT LIGATURES.**—Catgut is the best possible material for ligatures when it has been long steeped in carbolised oil. It is perfectly tough, flexible, and smooth. It excites no irritation, absorbs no putrescent fluid, and though it gradually melts away, yet holds the artery firmly enough to close its tube permanently. If it be softened too much, as it may easily be if steeped in water, it will relax and not hold the vessel permanently closed. If it be too little softened, as when merely passed through the mouth, it will probably be too harsh and hard. (Mr. T. Holmes, p. 176.)

**DISLOCATIONS OF THE HIP.**—*Reduction without Traction.*—The importance of fully understanding the anatomy and functions of the Y-ligament, commonly known as the ilio-femoral, cannot be over-estimated in order to a successful treatment of dislocations of the hip. A dislocation into the sciatic notch cannot be reduced by traction unless with the thigh flexed upon the body so as to relax this ligament, and reduction by manipulation also requires the same position. In a case of dislocation "vertically downward" in a child, after chloroform had been given the knee and hip were flexed and the femur rotated in; the head of the bone, however, slipped backwards towards the sciatic notch. At once the femur was rotated outwards, and the head slipped into the acetabulum. (Dr. R. Barker, p. 136.)

**FRACTURED CLAVICLE.**—Take a long and broad strap of good plaster (Maw's moleskin), pass one end of it round the arm below the axilla, and, having well fixed it to prevent slipping, pass the long remaining portion round the back, under the opposite axilla and round the chest, pinning its end to itself to prevent displacement. During this the arm must be brought well backwards until the tightened clavicular portion of the pectoralis major overcomes the sternocleido-mastoid, and thus pulls the inner portion of the clavicle down to its level. Another similar strap of plaster should now be made to support the elbow by passing under it and over the opposite shoulder, diagonally across the back and chest. (Dr. L. A. Sayre, p. 147.)

“PERSISTENT” INFLAMMATION OF JOINTS.—*Oleate of Mercury and Morphia*.—When pure oxide of mercury freshly prepared by precipitation is added to oleic acid it combines with it, forming an oleate of mercury dissolved in oleic acid. A small amount of pure morphia (gr. i to 3 j.) should also be added. A solution containing 5 per cent. is limpid and yellow, 10 per cent. dark, and 20 per cent. more of the consistence of an ointment, melting at the temperature of the body. Let the 5 per cent. solution be brushed over an inflamed knee-joint which has resisted other means of cure, and the mercury and morphia will be rapidly dissolved, arresting the inflammation and relieving the pain. As a rule, according to the size of the part affected, from ten to thirty drops are sufficient for one application. This should be repeated twice daily for four or five days, then at night only for four or five other days, and afterwards every other day until a cure is obtained. The diffusibility of oleic acid is much greater than that of ordinary oils or fats. The knee is merely taken as an illustration, the same plan is equally efficacious in inflammations of other external parts of the body. (Prof. J. Marshall, p. 138.)

REMOVAL OF THE TONGUE.—*Gag for the Operation*.—There are great difficulties in contriving an instrument which shall effectually retain the mouth open during this operation, and which cannot be got rid of by the patient during his struggles when under chloroform. A gag, invented by Mr. John Wood, of King’s College Hospital, seems everything which can be desired. It will be found fully described at p. 234. It may be, however, understood thus: Imagine two letter X laid down on the side and placed parallel to one another. The two antero-superior extremities pass under the bicuspid of the upper jaw on each side, the two antero-inferior extremities pass over and depress the bicuspid of the lower jaw. Separation is effected by screws working between the postero-superior and postero-inferior extremities of each side. A strap is passed round the back of the neck to secure it in its place. Each side can be worked separately. (Mr. J. Wood, p. 233).

SPIRAL SPRING EXTENDER FOR DISEASED JOINTS.—Mr. Holt-house of the Middlesex Hospital describes a spiral spring extender as used by him in cases of diseased joints. It is as efficient as Sayre’s splint and less expensive. It is on the principle of the Jack-in-the-box toy. The spiral spring surrounds the thigh, being wider at the upper than the lower end. A purchase is got upon the thigh by plaster and roller bandage over it, just as in Sayre’s splint, the tapes or web-



bing from these plasters being attached to the lower end of the splint by buckles. The larger end of the splint comes against the perineum, which forms the fulcrum. Any amount of traction on the hip joint may be obtained according as the tapes at the lower end of the extender are buckled tightly or loosely. (Mr. C. Holthouse, p. 145.)

**SPLINTS.**—*The Anterior Suspending Splint.*—The anterior suspending splint—the invention of Prof. Smith, of Maryland University—consists of two parallel wires kept an inch and a half from each other by four transverse fixed bars. It is adapted to the curves of the limb by bending it in the places necessary. The limb is then suspended to it by four or five short bands (see woodcut, p. 151). Several strong rollers should then be applied to the whole length of the limb, to replace the short bands, which are to be withdrawn as the roller is applied. The limb is suspended in the manner shown in the engraving. (Dr. C. Shrimpton, p. 150.)

*Sayre's Splint in Fractured Neck of the Femur.*—Sayre's splint is a capital instrument for fractured neck of the thigh bone. It has this great advantage over the long splint, that it does not confine the patient to a position of absolute recumbency, without change of posture, thereby avoiding the tendency to the formation of bed-sores and other discomforts. Moreover, the pressure by the perineal band is reduced to a minimum, because the extending force acts on the pelvic pad which is well pressed against the pelvis, and so, to some extent bears the strain. In the case in which I tried it the splint gave me great satisfaction; it kept the limb well *in situ*, prevented the common shortening and eversion, and was not inconvenient to the patient. (Mr. W. Colles, p. 148.)

*To apply a Plaster-of-Paris Splint.*—The plaster bandage is prepared by rubbing fine dry plaster-of-Paris into the meshes of a linen, flannel, or cotton bandage. Linen of loose texture is the best. Tarletan was the material most used by the Germans in the late war, and they strengthened their splints with thin wooden ribbons. The bandages so prepared may be kept ready for use in tin boxes. The limb should first be surrounded with blanket, neatly adapted to it and secured by a few stitches. The bandage is then to be applied, being first immersed for three minutes in hot water and salt. Should the plaster seem to dry during the application the hand, wet with water, should be passed backwards and forwards over it. Three or four bandages should make a firm splint from the toes to the knee. An elegant finish and some additional firmness may be given by smearing over the whole about two ounces of plaster dissolved in as much water.

The best time to apply the bandage is as soon after the accident as possible, even whilst the patient is suffering from the shock. (Mr. S. B. St. John, p. 124).

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### AFFECTIONS OF THE SKIN, &c.

**ACNE.**—Simple cases require little more than using no soap and washing in hot water. Advanced cases require first, a course of saline aperients, second, the administration of steel, except in cases where there is no particular weakness of system. Some cases even require alkalies. Thirdly, give arsenic as soon as the foregoing remedies cease to have any control over the disease. Few local remedies have any control over the disease—the best is the hypochloride of sulphur, the ointment of which should be smeared pretty thickly over the spots at bed-time. (Mr. J. L. Milton, p. 306).

**BRONCHOCELE.**—A swelling of the thyroid gland, big enough to be an extreme deformity and to cause stridulous breathing, will almost always gradually disappear under the steady and prolonged administration of the tincture of iodine, and of the tincture of sesquichloride of iron. No local application of iodine is necessary. The syrupus ferri iodidi is inert for this purpose. (Dr. J. K. Spender, Brit. Med. Journ., June 15, p. 636.)

**CHRONIC GLANDULAR ENLARGEMENTS.**—Muriate of lime, or as it is more commonly called chloride of calcium, is a remedy of considerable value in chronic glandular enlargements of a strumous character. Also in tabes mesenterica of children. It should be given for many months, twice or three times a day, after meals. A little milk conceals the taste better than anything. Ten grains three times a day may be considered an average dose for a young adult, but it may with advantage be increased to double that amount in some cases. The dose for a young child should be two or three grains. (Dr. Begbie, Ed. Med. Jour., July, p. 46.)

**CORNS.**—Pull out hard ones by the aid of a small sharp-pointed scalpel or tenotomy knife, and if well done the cure is often radical. Or a small plaster made by melting a piece of emp. plumbi, and dropping it on a piece of white silk, should be worn over the corn for a few days. This will cause it gradually to loosen its hold upon the subjacent healthy skin, and it can then be readily pulled or picked out. Soft corns should be treated by the use of astringents, such as alum dissolved in white of egg, or the careful application of tincture of iodine. (p. 312.)



**ERYSIPELAS.** — *Styptic-Colloid.* — Richardson's styptic-colloid, painted over the affected surface twice in the twenty-four hours, both relieves the suffering and arrests the progress of erysipelas. It has, moreover, the additional merit of having an agreeable odour and being cleanly. (Dr. G. F. Elliott, Brit. Med. Jour., Oct. 26, p. 466.)

**INGROWING NAIL.** — Make a small piece of sponge solid, by winding string round it tightly, and drying it. A small pyramidal piece should be inserted just beneath the nail, and secured by strips of adhesive plaster. The sponge will become moist and swollen, keeping the nail from the irritated flesh. Any granulations should previously be destroyed with strong nitric acid. (Mr. B. Blower, p. 313.)

**LOCAL INFLAMMATIONS.** — *Counter-Irritation with Iodine.* — Practitioners who have been accustomed to apply the feeble tincture of iodine externally, are surprised by the positive and splendid results often obtained by the use, as a counter-irritant, of the linimentum iodi of the B. P. This remedy is one of very old standing at King's College Hospital. It should not be applied over the inflamed part itself, but over the next vascular territory. If it be applied exactly over a bubo, or abscess, or carbuncle, or boil, the inflammation will be increased, but a circumjacent zone of vesication will remove the inflammation; the completeness and rapidity with which this is done being dependent on the width, locality, and intensity of the vesicating patch or circle. In a case of threatening strumous disease of the left shoulder-joint, with much pain and swelling, and some pyrexia, a broad ring of iodine liniment was painted over the lower part of the neck, the adjacent portion of the thorax, and the superior third of the upper arm; the whole skin area of the joint being left intact within the circle of counter-irritation. The liniment was carefully applied on alternate days, and the forearm supported in a sling. The constitutional treatment consisted of the administration of citrate of iron, iodide of potassium, and cod-liver oil. There was no interruption to the favourable progress of the case. All the joint-symptoms gradually disappeared and complete recovery ensued. (Dr. J. K. Spender, Brit. Med. Jour., June 15, p. 636.)

**PEDICULI.** — A solution of the oxide of mercury in excess of oleic acid, by its great penetrating power destroys at once both pediculi and ova, a result not always certain when ointments are used. (Mr. J. Marshall, p. 144.)

**TUMOURS.** — *Electrolytic action of the continuous Electric Current.* — The action of a continuous electric current upon a tumour

is simply that of a painless caustic. It does not cause a withering and shrinking of the growth, but a slough, precisely as does any other caustic. It has, however, the great recommendation of being very handy as well as painless. A Stöhrer's battery is very suitable, with twelve cells, and using two needles, the one at the positive the other at the negative pole. Small growths may be rapidly destroyed without complaint of pain on the part of the patient. For the cauterising of small nodules which may spring up about the scar-tissue after removal of a cancer growth, and which if allowed to increase, necessitate for their removal a considerable operation, the continuous current offers an efficient remedy. It does not seem, however, to promise much in the case of large growths. (Mr. G. W. Callender, Brit. Med. Journ., Feb. 10, p. 149.)

**ULCERS OF THE LEG.**—Having removed all hypertrophied cuticle at margins of the ulcer, it should be hermetically sealed by the application of oiled silk, collodion, and strapping in the following manner: A square piece of soap-strapping, two inches larger than the outer circumference of the ulcer, having a hole made of the exact shape of the ulcer, is applied to the leg; upon this strapping good collodion is applied with a brush, and over the ulcer and strapping one square piece of oiled silk is laid. This at once seals the ulcer, and in order to prevent the edges of the oiled silk from rubbing up they should be further fastened down with small strips of plaster. If the discharge be profuse the sealing will require to be repeated in a couple of days, but the intervals of removal are quickly prolonged, inasmuch as the exclusion of air limits the discharge and facilitates the formation of skin from the edge of the ulcer. No other application is required, and the ulcer speedily heals. (Mr. B. Holt, p. 312.)

*To heal an Ulcer on the Leg without Confinement.*—Let the patient apply, for several hours, over the ulcer, an ordinary lead lotion, containing a very small amount of carbolic acid, say thirty minims to the half-pint. On removing this, let the wound be cleaned from all scab and surrounding dirt, and let the lotion be reapplied for a few minutes. Then dry the surrounding skin with a piece of soft linen, and let the patient sit or lie for some hours with the parts freely exposed to the air. The surface will become dried and glazed, and in fact, covered with what is practically an impervious membrane. Let the patient go on with his business as usual next day, having the part covered only by the loose trousers. If the ulcer is large or has been suppurating profusely, it will generally be found that suppuration has taken



place under the membrane, if we may so call it. In that case the greenish white colour is visible through it. The process must then be repeated. Next night the suppuration will have occurred, and the sore will gradually dry up and no further treatment be required. Cicatrization will go on rapidly underneath, and the membrane, will in time assume the appearance of a thin scale or scab, will drop off, leaving a healed but rather depressed surface. (Dr. J. Braithwaite, Brit. Med. Jour., June 1, p. 581.)

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### VENEREAL AFFECTIONS.

**SYPHILIS.**—The law of syphilis is the same as that of small-pox, cow-pox, or measles. A man cannot have a fresh attack so long as the disease remains in the constitution. For instance, after indurated chancre and the appearance of secondary symptoms, it is not possible for the patient to contract a new indurated chancre, with swelling of the glands, manifestation of skin disease, and so on. If, perchance, something of the kind takes place the symptoms do not follow the regular evolution. (M. Ricord, p. 314.)

As soon as we have ascertained that there is a hardened chancre with swelling of the glands—not inflammatory, because the glands in this case never suppurate—the mercurial treatment should be begun at once. If this treatment is soon begun and well carried through, the bursting out of the first secondary symptoms, the roseola, the swelling of the glands of the neck, &c., may be prevented. If this is not frequently the case it is because the treatment is resorted to too late, when the disease has had time to take hold. In such cases, if the treatment is steadily pursued, the secondary symptoms, if they have shown themselves, soon disappear. It is, however, necessary to continue the treatment in some cases five or six months, having regard at the same time to sustaining the system in general. If this is done there are very few cases of relapse. If no symptoms have re-appeared after six months of mercurial treatment, the treatment with iodine should be begun and continued for five or six months more. The protoioduret of mercury is the best mercurial preparation to use. (M. Ricord, p. 315.)

**SUBSTITUTE FOR THE MERCURIAL BATH.**—A twenty per cent. solution of oleate of mercury in oleic acid is of the consistence of an ointment. There is no more effectual, cleanly, and unobjectionable method of bringing the system under the action of mercury than the placing of a small piece of this ointment in the axilla, night and morning. (Mr. J. Marshall, p. 145.)

## AFFECTIONS OF THE EYE AND EAR.

**AMAUROSIS.**—*Subcutaneous Injection of Strychnia.*—The subcutaneous injection of strychnia seems proved to be of great service in the treatment of amaurosis. In an illustrative case the amaurosis had existed two months and a half, and was getting worse. For five weeks strychnia was injected twice a week into the skin of the temple, commencing with  $\frac{1}{40}$ th of a grain, and increasing it up to  $\frac{1}{10}$ th. The case could not be said to be cured, but so much improved that the patient was able to do his work (watchmaking) with ease. (Mr. C. Higgins, p. 281.)

**ATROPIA IN OPHTHALMIC PRACTICE.**—We could better afford to dispense with all other drugs, lotions, and applications put together, than with this one topical medicament. “It is as safe in ophthalmic practice to use an atropine drop when in doubt, as in whist to play a trump.” There are only two absolute contraindications, viz., in the existing oval dilatation of the iris in glaucoma, and in peripheral wounds of the cornea. The most useful strength is expressed in the following formula: Neutral sulphate of atropia, two grains; glycerine, five drops; distilled water, an ounce. (Mr. E. Hart, p. 283.)

**AUDITORY VERTIGO (MENIERE’S DISEASE).**—There are cases which simulate apoplexy very closely, but are really owing to disease in the internal ear, probably in the semicircular canals. It is thought that these canals have no connection with hearing, but form the organ of the sense of equilibrium of the head, and thereby of the whole body. In the milder cases there is simply tinnitus and deafness attended with vertigo. In more severe cases vertigo, vomiting, tinnitus, and a fainting condition, leaving behind a certain impediment in motion, and unsteadiness in standing and walking. (Dr. J. Hughlings-Jackson, p. 75.)

**CONJUNCTIVA.**—*Preservation or Destruction of in Extirpation of the Globe.*—It is not in every case advisable that the conjunctiva should be left permanently intact in cases of extirpation of the globe of the eye. The patients may object to wear a glass eye on account of the trouble or the recurring expense, or he may experience such trouble from the conjunctival discharges that he may wish the annoyance removed if possible. In these cases, and also where from the existence of prominent bands of tissue it is impossible that a glass eye can be worn, it is better to destroy the conjunctival cavity altogether. This cannot be done by the knife. A thin paste of chloride of zinc should for this purpose be smeared over the



conjunctiva, and left there a sufficient length of time to destroy completely the mucous surface. It should then be all thoroughly washed away, the lids well closed, and the opposite surfaces allowed to heal. Any small spaces still left should be got rid of by the use of a hot iron. (Mr. J. F. Streatfeild, p. 263.)

**FAULTY EYELASHES.**—*To Eradicate.*—Firmly compress the eyelid between the blades of a suitable entropion forceps. Next pass in the needle of a hypodermic injection syringe at the free edge of the lid, as closely as possible to the hair follicles, and inject an extremely small quantity of tincture of iron. (Mr. P. J. Hayes, p. 261.)

**FOREIGN BODIES IN THE EAR.**—*To Extract.*—Surround a stem delicately with lint, the end of which a little unravelled is dipped in carpenter's glue. None of the glue should touch the outside of the cup of lint. The stem must now be passed down to the foreign body, and left in contact with it from fifteen minutes to an hour, and the fastening then becomes so firm that the foreign body can be extracted by careful management. (Dr. Löwenberg, p. 290.)

**OPACITIES OF THE CORNEA.**—*To Tint.*—Opacities of the cornea, whether partial or complete, are often a serious trouble to young persons on account of their unsightly appearance, more so on this account perhaps than on account of the defect of vision produced. The opacity may be tinted with Indian ink, sepia, or ultramarine, according to the colour of the iris or the situation of the opacity. For this purpose a number of the finest needles should be firmly bound together with their points on a level around a handle, and the part tattooed. When an immediate and deeply-coloured effect is desirable, a combination of lamp black with Indian ink, and a solution of nitrate of silver may be used. (Mr. C. B. Taylor, p. 259.)

**PURULENT OPHTHALMIA IN ADULTS.**—Hard chemosis overlapping the cornea should be freely incised. Astringents should not be used until the acute inflammatory stage has subsided. Alum and tannic acid are the best. Much harm is done by the use of escharotics; they add to the already existing irritation, prolong the disease, and damage the structure of the conjunctiva and that of the cornea. (Mr. H. Walton, *Med. Times and Gaz.*, Oct. 26, p. 456.)

**STRUMOUS OPHTHALMIA.**—Belladonna given internally in doses of one-eighth to one-quarter of a grain of the extract is a remedy of the greatest value in the treatment of cases of strumous ophthalmia—diminishing the photophobia, and in

conjunction with proper constitutional and local remedies curing the disease, when other means have failed. The best local application is perhaps atropine in a two or four-grain solution. (Dr. H. Power, p. 251.)

**SYRINGING THE EARS.**—There is a skilful and unskilful way of doing this. The auricle should be drawn upwards between the two fingers of the left hand, so as to put the whole meatus as far as possible in a straight line, and the nozzle of the syringe should be kept in close contact with the roof of the meatus. If the secretion is hard, it should be softened by a little warm water poured in for a few nights previously. (Mr. W. B. Dalby, p. 287.)

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### MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

**ARTIFICIAL AMNION BAG.**—In cases where the waters have been early evacuated, but the os is hard and unyielding, or in cases of placenta prævia, when rapid dilatation of the os is necessary, the artificial amnion bag of Dr. Morgan is likely to prove invaluable. It is pear-shaped, and considerably larger and stronger than Barnes's largest dilator, which it otherwise resembles. An unyielding material is contained at the junction of the ball and tube, extending a little upwards and downwards in order that this part may not give way when traction is made upon the ball by means of the tube. The ball is inserted well inside the os, and then dilated with water or air. The head of the child descends during a pain, compresses the upper part of the dilator, which, in expanding below, dilates the os. This is further assisted by traction upon the tube. The instrument is one of great value. (Dr. H. Morgan, p. 319).

**CHRONIC INVERSION OF THE UTERUS.**—A uterus inverted for some weeks can very rarely be reduced suddenly. Not only can it not be done, but if attempted, even under chloroform, dangerous collapse will ensue. The best way of proceeding is the following: A ring-pessary, of India-rubber, capable of inflation, and large enough to surround the prolapsed and inverted fundus, is placed so surrounding it and inflated. This steadies and supports the uterus and upper part of the vagina under the pressure of the second pessary, which is globular, and is inflated after introduction just like the first. This presses continuously upon the fundus, being aided by a T-bandage, with a large perineal pad. These should be removed and the passages washed out next day, and then be reintroduced. If after two or three days the uterus is not



restored take a vulcanite stethoscope, cover the chest end with an elastic pad, and press this against the inverted fundus as nearly as possible in the axis of the organ, that is, with the stem pushed back to the fourchette. A T-bandage secures constancy to the pressure. This with a little management and perseverance usually succeed in restoring the natural relative position of parts. (Dr. J. B. Hicks, p. 373).

**ELEVATION OF DEPRESSED CRANIAL BONE BY PNEUMATIC TRACTION.**—A child was born with its right frontal eminence occupied by a depression an inch and a half in diameter. It had been driven in during birth by the somewhat angular pubic arch of the mother. A most ingenious, and as it proved, successful plan of elevating the depressed bone was pursued. A cupping glass was placed over the part, and complete exclusion of air ensured by a cell of glazier's putty. Exhaustion was then effected by a small air-pump. The depressed bone gradually rose and assumed its normal contour. (Mr. H. W. Larkin, p. 156).

**DYSMENORRHOEA.**—*Spasmodic or Mechanical.*—There are many strong arguments against the mechanical theory of some cases of dysmenorrhœa. The value and application of the treatment by dilatation have, however, no essential dependence whatever on the truth of the theory which led to its adoption. It seems probable, however, that the use of any cutting instrument is likely only to be followed by ultimate induration of the canal or passage by the resulting cicatrices. This, however, is yet to be decided by experience. A plan more to be recommended is the introduction of bougies, gradually increasing in size, until one of No. 18 or 19, in the ordinary male series, can be passed. The bougie requires a little pressure for about a minute in order to cause it to pass the internal os. The dysmenorrhœa-like pain produced is severe, and lasts for about fifteen or twenty minutes, and then the bougie is withdrawn. This plan of treatment causes no injurious inflammation and is not likely to do harm, if it does no permanent good. (Dr. J. M. Duncan, p. 344.)

**ENLARGEMENT OF THE UTERUS.**—Enlargement of the uterus can only be diagnosed by means of the uterine sound, which is not only one of the most useful, but one of the safest of instruments. Enlargement of the uterus may be the result of subinvolution following abortion or labour, of congestion from retardation or suppression of inflammation, of acute or chronic inflammation of the uterus, or of the presence of fibrous tumours or polypi. Besides these cause-cases of simple hypertrophy of the uterus occur. The organ

slowly increases in size. Sometimes the body of the uterus is painful to pressure in these cases. (Dr. Atthill, p. 353.)

[We should have thought the presence of pain upon pressure diagnostic of the existence of metritis, and that in hypertrophy unattended with inflammation that symptom would be absent. EDS.]

**FIBROUS TUMOUR OF THE UTERUS.**—*Situation of Attachment.*  
—When one side of the uterus is bulged out the pedicle of the tumour is in all probability attached at the opposite side to the bulging portion. (Dr. G. H. Kidd, p. 369.)

*Imbedded Uterine-Fibroid.*—When, after dilatation of the internal os, the external os having been previously enlarged with scissors, it is found that a fibroid is imbedded in the uterine walls, although projecting to some extent into its cavity, what is the best plan of proceeding? The case published by Dr. Whiteford, of Greenock, speaks very strongly in favour of Dr. Matthews Duncan's operation, viz., simply dividing with a knife the capsule of the tumour by a crucial incision. This sets free the tumour, which is gradually enucleated by contraction of the uterus, and finally expelled with pains as in abortion. In Dr. Whiteford's case all hemorrhage ceased from the time of the incision, Six months after the operation, regular labour pains came on, and the tumour, which was three or four inches long by one inch in thickness, was expelled. There was a serous oozing, but no other form of discharge during this six months, and the tumour when expelled was not putrescent. (Dr. J. Whiteford, p. 371.)

**IMMEDIATE TRANSFUSION OF BLOOD.**—The author relates a mode of directly transfusing blood from the arm of one person to that of another, which will be at once understood by reference to the woodcut at p. 339. The ball contains only two drachms of fluid, and no valves, which whip the blood when passing through them. The ball and tubes are carefully filled with warm water, which is injected into the patient's circulation by the first compression of the ball. Instead of valves the tubes are to be compressed at one side or the other as necessary. (Dr. J. H. Aveling, p. 337.)

**INERTIA OF THE UTERUS.**—*External Pressure.*—If during labour, when the os is fully dilated, the uterus remains in a state of inertia, let the accoucheur (or some intelligent woman) pass the left arm under the patient and the right above, embracing the uterus between them in a direction parallel with the axis of the brim, interlocking the fingers and spreading them over as large a portion of the fundus as



possible. The pressure exercised is to be intermittent so as to imitate the normal uterine contractions. (Mr. L. Tait, p. 342.)

**LACTO-PHOSPHATE OF LIME IN MINERAL INANITION.**—The importance of phosphate of lime in the animal economy has scarcely been hitherto appreciated. So necessary is it that if deficient the tissues even draw from the osseous skeleton that which is necessary for the maintenance of the integrity of the functions of nutrition. If animals are artificially deprived of phosphate there is a loss of appetite and general debility. Ordinary phosphate of lime is soluble to a great extent in a healthy stomach, but when the digestion and vital power of the system generally is impaired it becomes dissolved with difficulty, or not at all. It may, however, be dissolved artificially in lactic acid, forming a lacto-phosphate of lime, which is easily assimilable. The most convenient forms for administration are Dusart's Lacto-Phosphate of Lime Syrup and Wine. Fifteen grains of the lacto-phosphate is contained in each table-spoonful of syrup. (Dr. Blache, p. 383).

**MAMMARY ABSCESS (THREATENED).**—*Application of Oleate of Mercury.*—Apply a solution of oleate of mercury and morphia in oleic acid, simply brushed over the part. The mercury is rapidly absorbed and arrests the inflammatory action, the morphia at the same time relieving the pain. (Mr. J. Marshall, p. 144.)

**"MARASMUS" OF CHILDREN.**—*Pancreatic Emulsion.*—In those wretched cases in children, in which every part wastes except the abdomen, pancreatic emulsion in doses of a teaspoonful every four hours is often of signal benefit. It is, of course, necessary that a proper diet should be insisted on at the same time, but proper diet without the pancreatic emulsion will not do. (Dr. Dobell, p. 388.)

**PERUVIAN BARK *versus* ERGOT OF RYE.**—Peruvian bark has a physiological and excitant action on the gravid uterus, and should in deficient pains be preferred to ergot of rye. It may be employed instead of ergot in hemorrhage during gestation or labour. (Dr. A. Monteverdi, *Lancet*, June 8, p. 808.)

**PLUGGING WITH COTTON WOOL.**—It has lately become the fashion to plug with cotton wool, and it certainly possesses great advantages both for introduction and removal. It is not, however, an efficient plug, because when wet it occupies a smaller space than it does when dry. Sponge is the reverse of cotton, it expands when wet, and fills the vagina more

completely. It is a most efficient plan to plug the os uteri alone with prepared sponge. (Dr. G. H. Kidd, p. 362.)

PROCIDENTIA UTERI.—In a case of complete procidentia in which no form of pessary could be retained, the uterus was placed *in situ*, and the actual cautery applied about an inch and a half from the vulva round the surface of the vagina for the extent of about half an inch. A good cicatrix resulted, which prevented the descent of the os. (Dr. E. Kennedy, p. 361.)

PUERPERAL MANIA.—*Chloral Hydrate*.—In a case of puerperal convulsions terminating in mania, in which there had been no sleep for five days, and in which during that period there had been not the least cessation of the maniacal symptoms, twenty-five grains of chloral procured a refreshing sleep of three hours; and a repetition of this treatment for a few nights effected a complete recovery. (Mr. F. V. M'Dowell, p. 341.)

SPHINCTERS OF THE UTERUS.—Both the external and internal mouths of the womb have their sphincters, as have all the other cavities of the body, the bladder, rectum, and stomach. There is not a separate band of muscular fibres at the os internum forming a regular sphincter, but the circular fibres of that part contract vitally and perform the functions of one. Closure of the orifices is the rule, a patulous condition implies disease. The os internum closes by contact with a cold body, such as the uterine sound, but this cannot be called stricture, and should not be treated as such by ruthlessly dividing it. A wax bougie would often pass where the sound would not. (Dr. J. H. Bennet, p. 321.)

SPINA BIFIDA.—Dr. Morton, of Glasgow, reports a case in which the plan of puncturing with a grooved needle, and subsequently injecting with a solution of iodine in glycerine proved successful. The punctures made with the grooved needle were closed with collodion after a portion of the fluid had escaped. These punctures were made at intervals of a few days, and about half the remaining fluid was drawn off with a fine trocar at the time of injection with iodine. (Dr. J. Morton, p. 397.)

STARCHY FOODS.—*Inability of Infants to Digest*.—If a quantity of saliva of an infant three or four months old be collected and mixed with a suitable amount of starchy food, it will be found, by the application of Trommer's test, that the saliva has absolutely no power of conversion of the starch into sugar. By experiments upon the pancreas of young animals

it is also found that the pancreatic juice is also devoid of starch digesting power. The importance of these facts on the feeding of infants with starchy food, as is so frequently done, will be at once seen. Even if they could be rendered digestible, starchy foods are not the best for infants. The growing infantile organism needs a greater quantity of those nutriments which afford directly materials to the development of the tissues than do starchy foods. Moreover, the only possible use which starchy foods could be, viz., to afford fuel to maintain the heat of the body, is better fulfilled by fatty matters. (Dr. P. Sonsino, p. 384.)

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## MISCELLANEA.

**ANÆSTHETICS.**—*Methylene Ether.*—It is probable that the best form of anæsthetic will turn out to be a combination of ether and Dr. Richardson's bichloride of methylene. Ether is too slow, although free from the danger of chloroform. Chloroform is effectual and rapid, but sometimes dangerous. The combination of ether and methylene is now the last recommendation of Dr. B. W. Richardson. A mixture of pure methylene bichloride and absolute ethylic ether, having a fluid specific gravity of 1.100. The vapour is agreeable. It is best administered from a simple mouth-piece made by Messrs. Krohne and Sesemann. From three to six fluid drachms is the average quantity required. (Dr. B. W. Richardson, Med. Times and Gazette, Nov. 23, p. 564.)

*Ether v. Chloroform.*—The odour of ether is disagreeable, it is slow in operation, and it gives rise to more excitement than chloroform; but *it is safer*. Chloroform, as may be shown by the hæmadynamometer, first raises and then depresses the heart's action. Ether raises the mercury of the instrument at once, and it never falls again. Ether in very large quantities produces no depressing action on the heart. (Dr. J. Morgan, Med. Press and Cir., July 31, p. 81.)

Although ether may be a little longer in acting than chloroform—and even this is not allowed by some of its advocates—yet, it is so incomparably safer, that it ought to be employed to the exclusion of its rival. It is unnecessary to have any admixture of air with the vapour of ether. Provided the patient breathes he cannot be too rapidly etherised, and there is no danger as in the case of chloroform from the vapour being too concentrated. The patient should not have taken food for three or four hours before, but he may have a cup of tea or a glass of wine. If this simple rule be



attended to, the cases in which sickness occurs will be exceptional. A thin hollow sponge, covered with a cone of pasteboard, is a good form of inhaler; and, upon the sponge about half-an-ounce of pure anhydrous sulphuric ether should be poured. After the first few inhalations, or as soon as anæsthesia is commencing, the sponge may be placed closely over the mouth and nose, and need not be removed, except when it is necessary to apply more ether. (Dr. J. Morgan, *Med. Press and Cir.*, Aug. 28, p. 165).

Ether, at the worst, is but a profound intoxication, and not unlike a drunken fit. It has always retained the first place amongst anæsthetics in America, never having been superseded by chloroform. In the legitimate use of ether by inhalation, no *bonâ fide* case of death is on record, or has ever taken place. It is absolutely safe, while the deaths from chloroform are numbered by hundreds. The failures in administering ether are attributed to dread of it; to the practice of administering it guttatim on a thin cloth, with a free admixture of air; to the removal of the towel on the struggle which announces the first stage of anæsthesia; and to the dread of stertorous breathing, and other signs of full etherisation. When the patient struggles, the ether must be held tighter over the mouth and nose, until they take long breaths and finally go off into ether-sleep. The less air mixed with the ether vapour the better. (Dr. Jeffries, *Brit. Med. Jour.*, Nov. 2, p. 500).

*Administration of Chloroform.*—All inhalers, except that of Clover's are bad. Out of 109 fatal cases an inhaler of some kind was used in twenty-eight. Clover's inhaler is the best, but even that possesses some disadvantages, the principal of which is, that in cases of very prolonged operation you are obliged to leave the patient for the purpose of refilling the bag. Chloroform was administered 5000 times at Edinburgh without accident, a simple folded napkin or towel being used. Nothing is better than a folded piece of lint, of three or four thicknesses, and of a size that can conveniently be held in the hollow of the fingers and palm. Evaporation from the upper surface of the lint is reduced to a minimum by holding the lint in the hand. The chloroform should be given at first with caution, and one finger placed on the middle temporal artery, the patient's face being exposed and watched. If swallowing, or slight spasm of the glottis occur, withdraw the chloroform and give it more gradually. The great thing required is undivided attention and caution. (Dr. G. V. Poore, *Lancet*, Oct. 26, p. 594.)

*Prevention of Death from Chloroform.*—Chloroform should never be administered without a rotatory battery, such as is in ordinary use, being kept ready at hand in case of accident. Should such threaten apply instantly one pole to the neck, and the other over the lower ribs on the left side. It will prove of little use if there is the delay of going to bring the instruments—seconds are of vital moment. Before this plan was adopted at the Bristol Infirmary there were three deaths; since its adoption in 1858, five lives have been saved by it, and no death has occurred. Pulse and respiration had ceased in three of the cases. It is a mistake to give chloroform to a person kept some hours without food, without some support to nerve-power, and brought to an operation under a dread of its consequences, it may be, shivering with cold—brought from a warm bed to a cold room. (Mr. T. Green, Brit. Med. Jour., May 25, p. 552.)

*APOMORPHIA AS AN EMETIC.*—Apomorphia is a product occurring in the retrograde metamorphosis of morphia. It is a valuable emetic, chiefly because it operates promptly; it may be administered subcutaneously, and indeed acts best in this way; and lastly it leaves no malaise or other ill effects behind. It should be injected into the skin of the abdomen in doses of .005 to .01 grain ( $\frac{1}{13}$  to  $\frac{1}{7}$  grain). In about three minutes the preliminary symptoms of its action come on, and in five minutes free emesis. No after effects remain longer than half-an-hour from the time of the injection. The hydrochlorate of apomorphia made by Macfarlane and Co., of Edinburgh, is the best. (MM. Riegel and Boehm, Glas. Med. Journ., May, p. 362.)

*DRINKING WATER.*—*To Ascertain the presence of Nitrogen in.*—Concentrate about two ounces of the water to one-eighth of its bulk, without boiling it. When cool pour a little into a test-tube to the depth of about an inch. Add an equal quantity of concentrated sulphuric acid. When quite cold, hold the tube nearly horizontally, and pour slowly in about an equal bulk of a pretty strong solution of green vitriol. After standing for half-an-hour, if a dark line is visible at the line of junction of the two liquids (which do not mix) the water does not contain less than .5 part of nitrogen in 100,000, though of course it may contain more. If the water has been derived from wells more than 100 feet deep, this may not show it to be contaminated—for the nitrogen is oxidised, and is an indication of previous sewage contamination. If the water is from a river or surface-well it is probably unfit for use. (Lancet, July 13, p. 46.)



**HYPODERMIC INJECTION OF MORPHIA AND ATROPINE.**—It is a good plan to combine  $\frac{1}{50}$  grain of sulphate of atropia with each  $\frac{1}{4}$  grain of morphia injected. This has a twofold effect. (1) It diminishes the tendency to nausea so constantly following the administration of morphia alone; and (2) it produces anodyne effects not to be obtained by morphia alone. If more atropine is injected symptoms of atropism show themselves—usually slight delirium or very rapid action of the heart. The effects of an over-dose of atropine can be counter-acted by the administration of more morphia. (Dr. J. M. Finny, Dublin Jour., July, p. 38.)

*Solution of Morphia for Hypodermic Injection.*—Dissolve 10 grains of hydrochlorate of morphia in two drachms of distilled water by the aid of heat, without any acid, spirit, or glycerine. Two minims of this solution, *i.e.* one-sixth of a grain, should be the commencing dose. It becomes solid at ordinary temperatures, and when wanted for use must be heated. The advantage is, that however long it is kept the solution never spoils. (Dr. H. Lawson, p. 61.)

**LENTIL MEAL.**—Lentil meal properly prepared and ground to a fine powder, and mixed with some other impalpable nitrogenous meal, such as rye meal, possesses many valuable qualities. It is highly nitrogenous and possesses four times the nutriment power of beef, weight for weight. It is very cheap—2½lbs. costs twenty-pence. The same quantity of the absurdly dear revalenta costs six shillings. It is so digestible that there is an absence of flatulence during the process. (Brit. Med. Jour., Sep. 7, p. 277.)

**NICOTINE AS AN ANTIDOTE TO STRYCHNIA.**—Professor Haughton, of Dublin, relates a case of strychnia poisoning which was treated by drop doses of nicotine in whisky punch every half-hour. At the second dose the paroxysms became less violent and the muscles of the abdomen softer. Four doses produced relaxation of the muscles. This confirms previous experience on this subject. Dr. Fraser, Professor of Materia-Medica, at Aberdeen, says that nicotine combines a feebly paralysing effect on the motor nerves, with a more decided paralysing action on voluntary muscular fibre. The action of nicotine very much resembles that of physostigma (or calabar bean), acting, however, more directly upon the muscles, and producing complete relaxation of them. Physostigma acts by diminishing, and in large doses destroying, the reflex activity of the spinal cord. (Brit. Med. Jour., June 22, p. 660.)

# PRACTICAL MEDICINE.

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## DISEASES AFFECTING THE SYSTEM GENERALLY.

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### 1.—ON TYPHOID FEVER.

By Sir WILLIAM GULL, M.D.

[The following is from a lecture delivered at Guy's Hospital, by Sir William Gull, on June 20th, this year. He remarked that it is 250 years since a king of England died of ague, a disease which now is comparatively rare, and foretold that typhoid fever would in another 250 years be equally uncommon, seeing that, like ague, it arises from preventible causes.]

Typhoid fever is stated to kill 17,000 a year in England; how great, then, must be the number attacked! It stood among the preventible diseases, and it was important, therefore, to know how it originates. There is no scientific theory, but there is a good working theory on the point. The origination of the disease is, somehow or another, connected with drainage. It has, therefore, been called the filth fever; hence to get rid of the filth is to get rid of the fever. It seems as if this really is so, for Millbank Prison was infested with typhoid and dysentery, but now the water-supply has been changed, and the drainage attended to, and these diseases have almost entirely disappeared. No one can approach a case of typhoid fever without paying some attention to hygiene. It is no use tinkering with the disease if one does not try to prevent it, and it no doubt may be prevented. The theory is that it is connected with germs which get into the blood; we know nothing about these germs—the air is full of them. There is an idea that they are imbibed by drinking water, and that they increase and multiply within the body. Although this has not been demonstrated, yet it is a good working theory.

The history of an attack of typhoid fever is somewhat obscure. The poison lurks in the body some time before the real onset of fever; it may be as long as five or six weeks. The disease then begins insidiously and irregularly; the prodromata are uncertain. Where does this poison work? Is it

in the intestinal tissue, or in the mucous membranes, or in the absorbent system? We do not know; we say that it is in the blood. Where are these lurking beginnings? These are probably in the intestinal system, for we find digestive disorders and disturbance of the bowels in the first instance. It is, however, very difficult to say when the disease begins; but if the ordinary course of health be represented by a straight line, we shall find at the commencement of the disease an abrupt deviation from this straight line, which deviation goes on increasing till the fourth to the sixth day; consequently, we find that on the fourth day the patient is ill, and on the fifth or sixth day very ill and obliged to keep his bed. It is about this time that we first see these patients as a rule, and how are we to tell that they have got fever? Well, by the little instrument called the thermometer. Now, if on even the first day the temperature be normal in the afternoon, there is no fever. In typhoid the temperature increases  $1.5^{\circ}$  to  $2.0^{\circ}$ , or even  $2.5^{\circ}$  F. in the first twenty-four hours. If a person who has been exposed to typhoid fever have a headache, and his temperature be  $101.5^{\circ}$  on the first day, it may be typhoid; but if, as is sometimes the case in persons who have been nursing, the temperature be below normal, or even if it be normal, you may be certain that there is not fever present. On the other hand, if on the first day the temperature be  $105^{\circ}$  you may be certain that the disease is not typhoid. We see, therefore, that whether the temperature be too low or too high the thermometer will greatly assist us in making a diagnosis. The temperature goes on rising about  $1.5^{\circ}$  per diem till about the fifth or sixth day, when it reaches its maximum of about  $103.5^{\circ}$  or  $104.0^{\circ}$ , or even more. This is not absolutely true, because some persons may go through typhoid fever and know nothing at all about it. In this disease, therefore, we see that there is first a long and uncertain prodromal stage, and then the onset of fever, which is insidious. Although the degree of temperature is reliable in determining the presence or absence of fever, the same cannot be said of the condition of the pulse, for, in nervous patients, the rapidity of the pulse may be greatly increased, and yet no fever be present. It therefore becomes an important thing to use the thermometer in practice; it will save many errors if used at the proper hour of the day—that is, in the afternoon.

To return to the course of typhoid fever, we find (still adhering to the chart) that on the twenty-first day the curved line suddenly runs into the straight line of ordinary health, and the fever is completed. In some cases, however, this does not take place till the twenty-eighth day. During this course we cannot give an answer to the queries of the patient's friends, "Is he better?" We must refer them to the chart, and tell



them what is the course through which the disease must go. "We shall find no word 'better' written anywhere on the curve." Without this is borne in mind much annoyance may be given to the friends, for in the morning the patient may be cooler and appear better, but in the evening he is hotter and appears worse, and if the medical man were to alter his opinion according to the degree of temperature the friends might say that the attendant knew nothing whatever about the disease. It is necessary to wait the natural term of the disease before we can say whether the patient is better; moreover, we must bear in mind that two charts of the course of the disease may be drawn out, one in which the disease is represented as lasting twenty-one days, and the other in which it lasts twenty-eight days. But it is not always easy to say which is the twenty-first or twenty-eighth day, as it is so difficult often to fix the date at which the disease began; but by carefully recording the temperature we may tell the time to a couple of days. On the other hand, it is not always that we can say positively the patient is better on these days, for the disease may, as it were, begin again, and exactly repeat itself, and this repetition may last twenty-eight days more, or it may even triple itself, as Sir William taught many years ago, when lecturing on this subject to the students at Guy's Hospital. This triple attack may last as long as the previous ones. These repetitions have been found on post-mortem examination to correspond with fresh outbreaks of the intestinal affection; for in these cases, should death occur, some of the ulcers will be found almost healed while others are just beginning to be affected. It is always necessary, therefore, to give a prognosis which shall allow of the possibility of these secondary attacks. But these repeated attacks do not necessarily last as long as the previous ones; they may be much shorter, lasting only one, two, or three weeks. So that typhoid fever may have a simple course of twenty-one days, or a prolonged course of twenty-eight days, or a double or triple course, the duration of which equals that of the previous attack; or lastly, a complex course—one or two attacks of ordinary duration, with another of shorter duration.

Now, in whatever manner the poison of typhoid is introduced into the system there is a concentration in the ileum. The presence or absence of diarrhœa depends on the implication or not of the large intestine. Although the ileum is the focus of the operation of the poison, yet it does not end here, for it is reflected to every organ of the body. The incidence of the poison is on the ileum, but the reflection is over the whole body. The body reacts to these conditions, and the fever is a reaction to the local condition of the bowels; but this does not imply that typhoid fever is a local affection of the bowels as

was formerly taught. This poison reacts in other ways, as by setting up ulceration in various parts of the body. He was once called to the case of a young girl who was sickening of an illness. She had a discharge from the vagina, which led the medical attendant to doubt the girl's chastity; but he (Sir William) said that it was typhoid, and that the discharge was due to ulceration of the vagina. Another case, in Guy's Hospital, was treated as a gonorrhœa; but in reality the patient had ulceration of the prepuce from typhoid fever. In a third case, the patient had perforating ulceration of the larynx, and he suddenly became emphysematous over the body, which "floored" him (Sir William); but this case was shown after death to be one of ulceration of the larynx occurring in the course of typhoid fever. This disease then is not a local one, although the focus of the action is in the ileum. But to return to the chart. About the ninth day of the disease mischief begins to show itself in the intestines, and at this time the deposit may open up a large bloodvessel, and thus give rise to severe hemorrhage, or, if it go still deeper, perforation of the bowel may take place; or the bronchial membranes may become affected, or the bronchial glands enlarged, which may give rise by reflex action to spasm of the larynx. Every organ in the body is affected if the disease be severe. Every part of the body may be affected through the nervous system. There is a tendency to reaction. There may be pleurisy; or the enlarged mesenteric glands may soften and suppurate, which may be followed by purulent infection; or the kidneys may become diseased. In fact, any organ may become affected through the action of the nervous, venous, or other systems.

Now as to the treatment. This disease cannot be cut short even in its early stage. Some have thought that it could be done by emetics, but few think so now. Others have thought that a purge of calomel and colocynth would cut short the attack, but this has been almost entirely given up. Others say that large doses of quinine will shorten the attack, or, at least, prevent serious complications; but Sir William related a case to which he was called, where large doses of quinine had been given, but the man was almost dead from hemorrhage, so that, in this case, the disease was neither cut short nor complications prevented. At the present time it must be granted that when once the patient has begun to go down the curve we must not dose him with medicines—we must not give him physic. The best thing is to place him in bed in a horizontal position, and reserve his nervous power, remembering that he must, if he live, go through the whole course of the disease. He has to go through the reactions of his nervous, absorbent, and nutritive systems; he has, in fact, to go through "a great physiological



storm." Let the patient alone then in this stage; "an early rest will save his power." But how is he to be treated through the remainder of the course? Is there anything to make the course less severe? Probably not. Quinine has been tried for this also, but has failed here as signally as when given to cut short the disease. It must be said that all the treatment consists of simple measures and simple diet. Medicines are only required for special conditions: for instance, if diarrhoea occur, some would give what is called "chalk mixture"; but we must remember that this has to go through about twenty-five feet of intestine. The best thing is to fill the rectum with five or six ounces of starch; it is not even necessary to put any opium with the starch. The diarrhoea only takes place when the rectum, sigmoid flexure, or descending colon is affected or irritated by acrid matters; hence, if you fill these the contact of noxious matters is prevented, and the diarrhoea checked. Time would not allow of more being said on this. [Here Sir William said he was not lecturing on typhoid fever, but only giving a few thoughts.] If hemorrhage occur, many would give gallic acid, or tannic acid or lead or some powerful astringent. But do not so; "don't begin with all your great guns at once." It is best to trust to the hemorrhage to cure itself, which it will do, and keep the patient at rest in the horizontal posture, giving a little opium (half a grain) by rectum or mouth if necessary to quiet the intestines. Ice may also be applied to the abdomen. This is sound advice, although it may appear terrible. The objection to giving lead or gallic acid or other powerful astringents is that they are apt to make the patient sick, and in that manner perhaps to make the hemorrhage worse. Often delirium comes on in the course of the disease. The patient may get out of bed, or even commit suicide by jumping out of the window. What is to be done for the delirium? In the first place never leave the patient alone day or night after he has begun to descend the curve, for delirium may come on at any time; therefore, he must be constantly watched. In the treatment of this complication remember that it is due to brain-irritation, and not to inflammation, as some have imagined. There is one great remedy for this, and it is alcohol; alcohol is the remedy for nervous irritation in typhoid fever, and, in fact, in any fever. How it acts is not certainly known, but it may be laid down that it is an important point in the treatment of all fevers. It allays nervous irritation and soothes the nervous system. Whenever, therefore, there is delirium or reflex irritation, give alcohol in some form or another, as brandy or wine; but when the result has been obtained, stop it; only give it when it is necessary. In fevers the use of stimulants is for the delirium, and it in no way alters the local processes, but only the reflex



condition. All cases do not require alcohol, and some only require it occasionally, and for a short time. Delirium even in the early stages may be checked by brandy, and never again come on during the whole course of the disease.

There is another point. Often the patient cannot or ought not to be moved out of bed. How is he to pass his stools? There is a danger of bedsores if care be not exercised to prevent them; but no man who has a due care ought, under ordinary circumstances, to have a patient suffering from bedsores; sometimes, however, the circumstances are such that these sores cannot be prevented. The bed-pan may become the source of a bed sore which may carry off the patient. Avoid, therefore, every possible cause of irritation or abrasion of the skin. This is a point which is worthy of the consideration of every clinical physician. Again, it may be necessary to pass a catheter to draw off the patient's urine. Now this may be done in such a manner as to injure the urethra, which may be in a very tender state, and cause ulceration and severe symptoms, and even death. Use, then, a soft catheter with all possible care and delicacy. In summing up he observed that the recovery from typhoid is dependent on attention to the smallest matters.

The time will come when this disease will become rare, or even die out.

We must save the patient's strength from the beginning, and not trust to medicines to cure the disease. The materia medica of fever is lessening every year.

Sir William would prefer to carry anyone through typhoid fever by wines and soups and fresh air, rather than by the use of drugs.—*Lancet*, June 29, 1872, p. 896.

## 2.—ON THE EXTINCTION OF TYPHOID FEVER IN THE MILLBANK PRISON BY THE DISUSE OF THAMES WATER.

By SURGEON-MAJOR A. C. C. DE RENZY, Sanitary Commissioner, Punjab.

It is instructive, as an illustration of the difficulty of discovering the true cause of sickness, to note that, of all the many distinguished physicians and men of science who were consulted from time to time about Millbank, no one appears for the first thirty years of its existence to have had a suspicion that the water-supply was in any degree answerable for the epidemics of fever, diarrhoea, and dysentery that visited the institution so frequently. During the inquiry conducted for many months by the two Select Committees of the House of Commons in 1823 and 1824, the state of the water-supply was not once mentioned. The earliest mention of any suspicion of

the water-supply is found in the Report of the Directors of Convict Prisons for 1852. At page 123, Dr. Baly says: "It was determined to put to the test of experiment an opinion more than once mooted respecting the cause, not only of the cholera, but also of the other kinds of bowel complaint which, in a mitigated degree, are almost constantly present in the Millbank Prison. It had been suggested, namely, that these diseases are produced by the Thames water, which the prisoners drink; and although for some years this water has been well filtered within the prison and freed from all obvious impurity, it was still possible that matters in solution in it produced a deleterious effect." It is evident from his later reports that this opinion had little weight with Dr. Baly; but, notwithstanding, he strongly recommended that it should be put to the test of experiment. His own opinion, which appears to have been shared by every physician who was consulted about the prison, was expressed as follows in his report for 1849, and repeated in several later reports:—"The causes of the general liability of the prisoners to fever," he says, "appear to be the low site of the prison, the proximity of low and ill-drained ground, open sewers and manufactories which fill the air with impurities, the construction of the building which impedes the free circulation of air through it, and the proneness to suffer from general causes of disease which is produced by the state of imprisonment." Dr. Baly was evidently surprised at the remarkable results that followed the introduction of a pure water-supply in August, 1864. He could not believe that the sudden cessation of cholera that followed in six days was due to the change of water-supply, but the continued healthiness of the prison in after years gradually shook his scepticism, though some traces of it remain apparent to the last. In his report for 1855, he writes: "One of the special tests of the health of Millbank prison is the degree of prevalence of diarrhoea, and there have been fewer complaints on account of this disease than in former years. This improvement in the health of the establishment began towards the close of 1854. It followed quickly upon the substitution of the artesian well-water for the Thames water, and it is, most probably, in great part due to the greater purity of the water at present in use. It may be hoped, therefore, that this improved health of the establishment will at all events to some extent be permanent. But it is not to be expected that the convicts in Millbank Prison will suffer as little from diarrhoea, dysentery, and fever, as the convicts in prisons differently situated." In his report for 1856 a further advance of opinion appears. He says: "I have again to report that the health of the prison is satisfactory, and that those diseases which have hitherto appeared to be inseparably connected with



locality have been less frequent. These favourable results are, I believe, attributable in great part to the purity of the water with which the prisoners are now supplied. I have, therefore, a strong hope that they will be permanent." It may here be stated that Dr. Baly mentions no other cause to which the improvement in the health of the prison could be attributed.

Dr. Guy, who succeeded Dr. Baly in the medical charge of the prison, expresses his surprise that, although the Thames was unusually offensive in 1859, the prison was unprecedentedly free from sickness. In 1862 Dr. Guy reports that "the male convict population are at least as healthy as any other population with which it can be compared, and that the female convict population suffers but little by comparison with the population out of doors." In 1871 the present able medical officer, Mr. Gover, reports that "the prisoners have continued to be free from every form of disease which could call the sanitary arrangements into question, as free as if the prison occupied the healthiest site in the kingdom." These extracts show very strikingly the permanence and the thoroughness of the change that has taken place in the health of the prison.

The sanitary history of Millbank appears to me to warrant the following conclusions:—

1. That the extinction of typhoid fever, and other diseases of the same class, is quite within the range of practicability.

2. That the extinction of one class of zymotic diseases is not necessarily followed by zymotic diseases of a different class. For example: it is supposed that the increased prevalence of scarlatina and measles of late years is due to the partial displacement of small-pox by vaccination. The case of Millbank shows that it is practicable to protect a community against every kind of zymotic disease. Ignorance of sanitary science is the great obstacle to the extension of this protection to the free population.

3. That since some of the ablest physicians in London failed for many years to detect the true cause of the unhealthiness of Millbank prison, and assigned causes for it which later experience has found to be unconnected with it, the probability is that a similar error is frequently made elsewhere, and that the prevalence of some zymotic diseases is ascribed to locality, malaria, heat, cold, variations of temperature, moral depression, and other intangible influences which would be entirely removed by the general disuse of impure water.

4. That as it required long years of observation to establish the noxious influence of Thames water in Millbank, even when well filtered, under conditions very favourable for detection, we should be cautious in accepting the opinion, based on the results of chemical analysis, that the use of that water by the population of London is free from danger.



5. That the vital statistics of prisons, carefully kept and tabulated, would be of the greatest value as data for the investigation of the causes of disease. Those now published by the directors of convict prisons are excellent. The sanitary information given in the reports of the Inspectors of Prisons is of the most meagre description, and of no practical use.—*Lancet*, June 15, 1872, p. 820.

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### 3.—THE TREATMENT OF ENTERIC, OR TYPHOID FEVER.

By DR. JAMES LITTLE, Physician to the Adelaide Hospital, Dublin.

Next to early confinement to bed, which perhaps more than anything else lessens the severity and risk of the fever, I rank the rigid exclusion of animal broths and jellies from the food, as tending to keep the disease mild. On this point I find myself quite at variance with the text-books in which such articles as beef-tea and Liebig's essence of meat are recommended. Dr. Hudson in his Lectures on Fever insists on the liability of all kinds of broths to increase the diarrhœa, and I cannot but attach the utmost importance to this matter, as I have repeatedly seen a patient who was passing four to six stools on a milk diet have eighteen or twenty during the day and night after he had taken beef-tea. Milk should be the chief article of diet in enteric fever. Thirsty patients sometimes object to its mawkish taste, and I generally, therefore, add ice to it, and a little lime water in cases in which it returns curdled. Junket or rennetted milk given before it has separated into whey and curd, rice milk, custard, baked custard in small quantity, rusks and hot milk, and blanc-mange, generally afford sufficiently varied ways of giving milk. Freshly made chicken jelly is less liable than beef-tea to increase the abdominal symptoms, and I use it in those cases in which milk, even with lime water, disagrees, but in my experience such an occurrence is very rare, and when encountered is usually in a person chronically dyspeptic. For years I have made the administration of two or three cups of really good tea or coffee between daybreak and two in the afternoon a regular part of the treatment of every case of fever, unless there was in the state of the nervous system some evident contra-indication. I did this in consequence of the well-known observations of Dr. Parkes on the effect of coffee in increasing the elimination of urea in fever, and I am satisfied that both it and tea lessen drowsiness and prostration, and increase the secretion of urine; once or twice in the day they may be given, poured upon a well-whisked egg, and thereby an additional means of nourishing the patient is obtained. I agree

with those who consider alcoholic stimulants in any quantity seldom needed in enteric fever.

Cold baths I have systematically employed since Dr. Cuming in his Report on Medicine in this Journal (May, 1869), drew attention to their use in enteric fever. I have not given them at such short intervals as recommended by Jürgensen and by Bartels and Liebermeister; three, or at most four are given in the twenty-four hours. In the hospital we have full-sized baths, which run on castors, one of these is brought to the bedside half full of water at 75° Fahr., and if the temperature of the patient exceed 102° Fahr., he is lifted out and laid in the bath; he is kept in it for a period varying from five to fifteen minutes, and then quickly dried and put back to bed. The bath is usually most grateful to the patients; they often say it makes them feel strong, and after the evening bath they commonly fall into a sound sleep. I have used with manifest benefit where cooing and wheezing râles existed in the chest, and where deficiency in the percussion resonance posteriorly and mucocrepitus indicated postural stasis in the lungs; but I have never used it when there was hemorrhage from the bowels, or such pain as to justify the fear that peritonitis existed. There is sometimes slight chilliness in the extremities after it, and a little shivering. This indicates that the bath should not be a prolonged one, but does not forbid its use. Twice, however, I have considered it unsafe to continue the baths, once because a marked shivering followed, and once because the patient was alarmed by it. In cases in which the disease is running a mild course I only give one bath in the day, at the height of the usual evening paroxysm of fever.

By a dietary such as I have described, and by the systematic employment of the baths, I am convinced the severity and danger of enteric fever are greatly diminished, and the occurrence of any of the serious accidents incidental to the complaint rendered very rare; but I do not believe that its duration is appreciably shortened.

Besides these means there are others which I have found of material benefit when certain conditions were present.

When during the first eight days the face is flushed and there is headache, a high temperature and a thickly coated tongue, and when the evacuations, three or four in the twenty-four hours, are neither very large nor very liquid, a dose of calomel, from four to six grains, perceptibly lessens the heaviness of the fever. I have sometimes given the calomel a second time, after an interval of a day or two; but never oftener; it increases the evacuations during the twelve hours after its administration, but never in my experience sets up troublesome purging.



In enteric fever it is not uncommon to find a patient lying on his back, perceptibly impeded in his breathing, his abdomen tumid and projecting, but not markedly tender, and on enquiry it will be found either that the bowels have not acted for twelve hours, or that though the stools are frequent, only a very little fœcal matter with wind passes each time. In such a case great relief is obtained by giving a draught containing two drachms of castor oil, with one or two of turpentine. If for any reason the physician hesitates in ordering this, he may have a warm water enema administered, but the draught relieves much more completely.

In my experience poultices and fomentations have not proved so useful as their constant recommendation by authors led me to expect; their weight, and the bandaging necessary to keep them properly applied render them anything but grateful to most patients, and I now seldom employ them except when the abdomen is tympanitic, in which case the turpentine stupe is of undoubted utility.

Since I have kept my patients rigidly to the diet I have mentioned, I have not often found it necessary to give medicines to check looseness of the bowels; three or four actions in the twenty-four hours seldom do any harm; when it is necessary to interfere I have found by far the most useful remedy a pill, the use of which Dr. Hudson some years ago suggested to me, it contains one-sixth of a grain of carbolic acid, one-sixth of a grain of opium, and three grains of bismuth. This combination I have found so admirable for moderating diarrhœa and tympanitis that I have seldom employed any other; two, three, or four pills may be given during the day and night; of other remedies sulphuric acid is, I think, the best,

Hemorrhage from the bowels is also rare when milk diet and cold baths are employed; when it occurs, gallic acid, a scruple every second or third hour, and turpentine were the remedies upon which, until lately, I relied; in consequence, however, of the power of arresting hemorrhage, which ergotin has recently been shown to possess, when administered hypodermically, I some weeks ago made up my mind to try it in the hemorrhage of typhoid, as soon as a case presented itself, and as far as the experience of a single one goes, I have reason to be well satisfied with the experiment. On the morning of the fourteenth day of enteric fever, blood appeared in the discharges of a patient now under my care in the Adelaide Hospital; when I visited her at noon I found more than a pint of clots had come away, and while I was in the ward more than half a pint of pure blood, partly clotted and partly liquid was passed, at the same time the features became sunken, and the extremities cold. I then injected five grains of ergotin, and repeated the



injection at three o'clock. Within half an hour of the first injection about three ounces of blood escaped from the bowel, but none came again until four o'clock the following morning, when half a pint of clots escaped, and a third injection was used; the only other treatment employed during that day and night was the application over the ilio-cæcal region of an ice bag. There was no further discharge of pure blood; the three or four next stools were, however, blackish, and as a precaution three twenty-grain doses of gallic acid were given on each of the two following days.—*Dublin Journal of Medical Science*, May, 1872, p. 371.

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#### 4.—ON THE USE OF STIMULANTS IN SEVERE CASES OF FEBRILE AND INFLAMMATORY DISEASES.

By Dr. LIONEL S. BEALE, F.R.S., Fellow of the Royal College of Physicians; Physician to King's College Hospital.

It has been found that persons suffering from fever will bear alcohol without any narcotic intoxicating effect whatever being induced. It has been demonstrated that a large amount (upwards of twenty ounces of French brandy in twenty-four hours) does not cause delirium in fever patients, does not produce inflammation of the brain, nor increase bronchitis or pneumonia if present. Neither does the alcohol augment dyspnoea even when very urgent. On the other hand, it is a fact that patients suffering from fever complicated with severe and extensive inflammation of internal organs—lungs, pleura, pericardium—have progressed favourably towards convalescence while taking brandy at the rate of more than twenty ounces in the twenty-four hours.

[There continues, nevertheless, to be considerable difference between the amount of stimulants prescribed in the treatment of such cases by different medical men. Some never go beyond two or three glasses of wine for patients in whose cases others would prescribe very large amounts, having learnt that by this means the period of convalescence is shortened and the patient makes a better recovery.]

Considering the grave importance of this practical question, it seems to me desirable that it should be fully discussed; and at the risk of being considered tedious, I propose to pass in review some of the facts and observations which, as I pointed out long ago, justify the administration of alcohol, and enable us to form some notion concerning the precise manner in which this substance acts advantageously in some cases of fever. In this way I hope to be able to support and explain the favourable results of clinical experience. Of the scientific arguments

many are based upon minute observations of my own extending over many years, and which were carried out without reference to the question at issue, and indeed without the thought that they might at some future time assist in elucidating it.

*Objections to the Administration of Alcohol in Inflammation.*—It is interesting, if in some respects painful and depressing, to study the wonderful alterations which have taken place in our views concerning the nature and treatment of important general pathological changes. The process called *inflammation* lies at the root of many of the disorders—acute and chronic—from which civilised man and the higher animals suffer. Inflammatory action is, as it were, the point round which Medical theories revolve, and differences regarding the nature of the phenomena comprised under inflammation have led to divisions upon the most important questions of practice, and have caused the greatest differences of opinion regarding the proper treatment of disease. Inflammation is a subject which always excites intense interest, and even now the nature of the changes taking place cannot be discussed without much feeling. The calm necessary for the steady prosecution of scientific discovery is not unfrequently disturbed by the vehemence and warmth of debate as to the proper interpretation of observed facts.

The term “inflammation” involves increased action; and in all inflammations it is true that there is increased action. In order to combat this undue action and reduce the burning activity of the inflammation, we used to be taught to give remedies which depressed the heart’s action and reduced the patient’s strength.

But it has long been observed that many forms of inflammation are only seen in systems already reduced and exhausted by disease, misery, or privation. There are many cases in which frequency of pulse, violent delirium, extreme prostration, and all those symptoms known to accompany extensive inflammatory action, are associated with a general state of the system which can hardly be made lower than it is without great risk to life. Nevertheless, with such confidence was the truth of the old combustion theory of inflammation believed in and taught, that the efforts to quench the fire or to moderate its intensity so absorbed the attention of the physician, that there was danger of losing the patient ere efforts employed to check the disease could prove successful.

Oftentimes in medicine and in science have facts been explained by theories which had never been deduced from the results of experiment; and when new facts opposed to a theory had been demonstrated, men have sometimes said the facts could not be true, and have persisted in acting upon the theory; at the same time appealing to the dogmas upon which the



theory was based to confirm them in the action which, it is to be feared, they had already determined to take. In days long gone by, stimulants had from time to time been given by intelligent Doctors, in low conditions of the system accompanied by local inflammations; and in many cases when the patient felt better after taking wine, Practitioners in olden time have even allowed a repetition of the practice, although they felt conscious it was against what was regarded by them as sound principles of treatment which they dared not doubt. The favourable action was ingeniously explained by the discovery of some idiosyncrasy or peculiarity in the constitution of the individual patient, instead of being attributed to changes consequent upon the action of the stimulant upon the phenomena of a particular abnormal state.

*Changes in Practice.*—By degrees, however, it came to be observed that stimulants seemed to act favourably in very many cases in which their administration was quite opposed to theory, and was in direct antagonism with the doctrines then taught; and at last it was admitted that experience was to be trusted, and that the doctrines formerly taught could not be true in all cases. Still more recently scientific observation and experiment have demonstrated that facts which had been appealed to had been misinterpreted and misunderstood, and that a plan of treatment at variance with the one formerly popular, and in harmony with that now followed out, was really indicated. No one who knows what changes are taking place in fever or inflammation would say that he objects to the exhibition of stimulants *because* some kind of inflammation or local fever is present. In fevers, which are in reality but general inflammations, the pulse has been observed by hundreds of Practitioners to diminish in frequency, delirium to give place to calm consciousness, and the feverish state cease while the patient is taking stimulants. Forty years ago such conditions would have been treated by bleeding, calomel, antimony, and lowering remedies.

Dr. Graves, of Dublin, who, like Todd, had been a teacher of physiology, advocated, as long ago as 1833, support in the treatment of fever; and a stimulating system had been carried into practice by Dr. Blakiston during the epidemic of influenza at Birmingham, about the year 1837. But it was reserved for Dr. Graves' pupil, Todd, to carry out this stimulating plan of treatment to its fullest extent, and to apply it more generally. During many of the earlier years of his life, Dr. Todd treated cases of acute disease like most Practitioners of that day; and in his oldest case-books are records of cases of acute pericarditis which were bled and treated by mercury or salivation; cases of pneumonia which were treated by bleeding and tartar emetic;



cases of fever in which a supporting plan was very hesitatingly and very imperfectly carried out. Slowly and gradually his treatment was much modified; and at length he became a strong opponent of the doctrines upon which the so-called *proper treatment* of inflammatory action was supposed to be based. Pericarditis and peritonitis were treated with opium without the mercury; stimulants were given, and the lancet was completely laid aside. Pneumonia was combated by counter-irritation and soothing poultices, and the skin and kidneys were made to act freely. The strength was supported; nourishing food was given; and if the powers of the patient flagged, brandy was administered, at first in small doses, but in many bad cases it was increased to considerable quantities. Desperate cases of low fever and extensive internal inflammation were treated by very large quantities of stimulants, the amount being varied from time to time according to the symptoms present and the progress of the case; but the proportion was not limited by any inflexible or arbitrary rule.

It is, however, only during the last twenty-five years that we have actually demonstrated that alcohol administered in small and oft-repeated doses does not *excite or increase the inflammatory process, and that inflammation may cease while a patient is taking considerable quantities of alcohol*; but even to this day there remain not a few who are not convinced of the truth of these remarks. Some have fallen into the error of supposing that Practitioners who advocate the use of stimulants in fever, order large quantities in every case. Practitioners have been accused of giving brandy in a routine manner, inconsiderately and indiscriminately. But this is a very grave charge to make, and ought to be supported, by those who prefer it, by reference to special cases. Mild cases of fever and inflammation were treated by Dr. Todd without stimulants altogether, or with very moderate quantities; but as this Physician was naturally desirous of treating as many desperate cases of febrile disease as possible, a very large number of the worst forms of acute disease admitted into the Hospital were placed under his care. The curious argument was adopted by some, that because very large quantities of alcohol were administered in some exceptional cases, equally large doses were given by him in all cases.

Unfortunately the question of stimulation is one which has not always been considered upon its merits only. The zealous opposition to a particular practice upon religious, political, or moral grounds may, without due care upon his part, quite unfit a man for the investigation of the effects of that practice upon the tissues of the living body under the varying circumstances of health, disease, climate, age, rest, anxiety, labour, &c. Such opposition ought to be discouraged.

*Objections on account of the large quantity of Stimulants given.*—Many objections have been offered to the “enormous amount” of stimulants given; but these objections do not rest upon actual evidence, and the arguments adduced against the system pursued have been satisfactorily answered. It seems never to have occurred to some, who have not hesitated to state the exact quantity of alcohol which in their opinion should never be exceeded, that an amount which might be excessive if given to a person weighing six stone, would be but a moderate dose, and perhaps insufficient, in the case of one weighing three times as much. In this matter it is wonderful that people who pride themselves upon the practical tendencies of their minds, instead of allowing themselves to be influenced by facts and reason, should act as if every individual were exactly alike and had been cast in the same mould. Some persons are better treated without alcohol, while others, suffering from the very same disorder, require a good allowance of stimulant. The difficulties of explaining this and many familiar facts are great indeed—perhaps insurmountable in the present state of our knowledge. Many of us have remarked how readily some persons when exposed to contagion contract the disease, while others altogether escape, or if attacked, progress favourably in spite of circumstances the most adverse. Original hereditary defects affecting the organs of circulation and the nervous system, particularly weak heart, will doubtless account for some of the cases we have observed. There is reason to think that many children who die early might have reached old age if they could have been preserved up to the period of early youth; while there is no doubt that in other instances adolescence, or a still later period of life, constitutes the critical period when exposure to the influence of contagious poison might be more disastrous than at any other time of life. Such considerations must always influence our judgment in determining the proper treatment, especially as regards the quantity of alcohol. In the regulation of the amount of food for prisoners, the inmates of workhouses, hospitals, and charitable institutions, there is too great a desire on the part of the authorities to adopt uniformity; as if every individual required precisely the same quantity. The consequence is that a diet which is low for some is more than sufficient for others. Some will be half-starved while others will be well fed—perhaps over-fed. Some of our Hospital authorities are painfully inflexible in these matters, and are continually trying to discover a diet that shall equally suit patients of all ages and every kind of disease. The diet-tables are changed every few years, and the physicians and surgeons continually blamed for ordering extras. Under the uniformity system it is clear that to the little people and the light weights is accorded a not perfectly fair advantage in the



struggle for existence. It is of some importance for the physician, among other particulars, to take carefully into his consideration, when prescribing and regulating the proportion of food and stimulant, the weight and vigour of the individual patient. For although it is true that in proportion to their weight small animals require much more food than large ones, a heavy man should, as a general rule, have a more liberal diet than a light one; and in apportioning the quantity of stimulant to the sick, this fact must not be neglected. But it must at the same time be borne in mind that by habit and other circumstances some persons have been led to take, and hence require, a larger proportion of food and stimulants than others of the same weight.

In low diseases the quantity of stimulants required during a short period may be very large; indeed, the patient's life seems sometimes to depend alone on the frequent doses of alcohol (occasionally as much as an ounce or even two ounces an hour) which are poured into the stomach and taken up by the blood; and it is remarkable that as long as the case does well the stimulant seems to be absorbed almost as fast as it is introduced into the stomach: a little escapes in the urine, in the breath, and perspiration, but by far the larger portion is used up in the system, and in two or three different ways helps to keep the patient alive at a time when the disease places him in the greatest jeopardy.

It was pointed out more than twenty years ago, that in some cases the period of convalescence was much shortened; in cases necessarily fatal, life was prolonged; and it is believed that many desperate cases of low fever, pneumonia, acute rheumatism, &c., have been saved by the administration of large quantities of stimulants. If a collection could be made of some of the most serious instances of febrile diseases that have recovered under alcohol from the practice of a considerable number of medical practitioners, I feel sure that so strong a case would be made out in favour of this mode of treatment that it would be generally adopted. Moreover, there are cases which have injudiciously been "given up" by the doctor that have recovered under the administration of stimulants, and it has occasionally happened that the delirious state has been relieved by an act performed by the patient in his delirium. Dr. Winn sends me the following interesting remarks in connection with an instance of this:—"As regards the use of alcohol in disease, I learnt a lesson when a student which I have never forgotten. I was attending the Fever Hospital at Glasgow, a city which was then quite a hotbed of fever, and I went there with my mind fully imbued with the antiphlogistic theories. Imagine my surprise on finding that the chief remedy



employed, and successfully too, at that hospital, in the worst cases of typhus, was whisky in frequent and large doses. I can now recall one case in particular—that of a patient in a state of acute delirium, whose head became quite clear after imbibing a bottle of whisky in about twenty-four hours!”

It may then be regarded as certain, that in a number of cases advantage has resulted from giving very large quantities of stimulants. Several remarkable instances will be found reported in Dr. Todd's "Clinical Lectures"; but among the most striking are the two cases already alluded to, which have been recently recorded by Dr. Wilson Fox. Alcohol is the most powerful remedy we possess, and probably the only one by which we are enabled to save life in desperate cases of fever.

When considering the circumstances which lead to death in severe cases of fever, I showed that the failure of the heart's action, the alteration in the composition of the blood, and the obstruction of the capillaries, and the consequent interference with the process of nutrition, all contribute to bring about a fatal result. Now, in slight cases frequent doses of alcohol tend to prevent these occurrences, and when the attack is severe, to mitigate its severity, to postpone the occurrence of the dangerous phenomena, and to prevent them from progressing to such an extent as to render a fatal result probable.

Alcohol, then, exerts a twofold action when given in cases of fever. It acts *directly* upon the nerves of the stomach, and through these excites almost instantly increased contraction of heart. The less *direct* action of alcohol is much more complex, and occurs more slowly; for before it can exert the indirect influence I refer to, it must be absorbed by the blood and circulated with it to all parts of the body. Although I have drawn attention to the facts upon which my views concerning the important indirect influence of alcohol are based, the subject is of such vast practical importance that I shall offer no apology for passing in review the several scientific arguments, which it appears to me prove conclusively the way in which, by the judicious use of this most important remedy, the saving of life is effected. But first, a very few words concerning the direct action of alcohol.

1. *Direct Action of Alcohol in keeping up the Heart's Action, and promoting the Capillary Circulation.*

Now, as to the probable action of alcohol in saving life, and retarding a fatal result in desperate cases of fever and extensive internal inflammation, I will remark that its value in exciting the heart to more vigorous contraction is admitted by all. The fact is in accordance with the actual experience of many of the effect of stimulants upon themselves, and physiological experiment has proved that by the action of alcohol upon the nerves

of the stomach the heart's action is accelerated. The value of stimulants in promoting the capillary circulation has been recognised by several physicians, but Sir D. J. Corrigan especially has dwelt upon the great importance of giving alcohol in fever, because it maintains the flow of blood through the minute vessels of the tissues of the body. In his "Lectures on the Treatment of Fever," published in 1853, and containing the results of great experience and many years of careful observation, occurs the following passage, which deserves to be quoted at length:—

"Ask yourselves for what is it in typhus fever you prescribe wine? Is it for delirium? No. Is it to prevent its approach? Again, no. Do you give it for a dry tongue? Certainly not. What is it that, as you consider a patient's state, would lead you to think of its employment? Is it not the state of the function of circulation, taken as a whole, indexed to you by the pulse on the one hand, and by the state of the capillary system of circulation in the skin on the other?"

"It is for this you give it. It is the specific remedy directed to remedy the general lesion of the function of circulation, and hence in its administration you may give it whether there is or is not delirium; for delirium may be present or absent in a case requiring its exhibition for the function of circulation. You should give it indifferently whether the tongue is moist or dry; for the tongue may be either, and yet wine may be required; and hence the tongue's becoming moist is not an indication that you may dispense with its use, nor is its continuing dry a sign to make you discontinue it. You may give it with a soft abdomen, or with an abdomen tympanitic, for similar reasons. You are giving wine, recollect, as the specific remedy for the lesion of the function of circulation (remember always, comprising under this the capillary and cardiac circulation); and by the state of pulse, and changes in the colour of the maculæ, you are to judge of the necessity of continuing, decreasing, or augmenting its dose. Under its exhibition you will see the vessels of the conjunctiva contract, the maculæ become rose-coloured, and the patches of skin on dependent portions of the body lose their dark livid hue. Keep this then in mind—the lesion in fever for which you give wine, is the lesion of circulation; and if this function from debility require it, you must give it under all circumstances of derangement of other functions.

## 2. *Indirect Action of Alcohol on the Blood and on the Bioplasm of the Tissues.*

In order that a correct idea may be formed of the manner in which the indirect action of alcohol upon the constituents of



the blood and upon the bioplasm in fever and inflammation is effected, it is necessary to direct the reader's attention to some questions of scientific detail, some of which may appear only very remotely connected with the subject, but which are nevertheless important, partly because they indicate precisely the change which is effected by the agent, partly because they enable us to correct erroneous theories which have hitherto been acted upon. And first as regards the conclusions formerly taught, and still to some extent believed, concerning the value of bleeding in fever and inflammation.

*Of Bleeding, and the Life of the Blood. Vital Stimuli, and of the life-giving and renewing Theory.*—From the earliest ages those who have devoted themselves to the study of disease, have regarded the condition of the blood as of the highest importance. To a bad state of this fluid many ailments have been correctly ascribed. To alter this state was and is the main object of treatment. Modern research has confirmed the opinion so long and so generally entertained with reference to the high importance of a healthy state of blood; and modern practice is eminently conservative of this fluid. Of late this view, like every other correct doctrine, has been assailed and doubts cast upon its correctness, but it has not suffered from the attack.

The principles upon which the removal of large quantities of blood was advocated and carried out at various periods in the history of medicine have been proved to have been founded upon incorrect data. The old theories of inflammation, even yet maintained by some, have been completely upset by observation and experiment. The blood, which used to be freely drawn for the purpose of reducing an excess of action is now considered, and in cases of the very same class, to be absolutely necessary for the recovery of the patient. It has been proved conclusively—(1) that morbid changes, which were formerly supposed to be checked by bleeding, really continue in spite of it; and (2) that by violent bleeding the general condition of the patient is rendered much more serious than before. Formerly a patient was bled to *cut short the disease*; now he is bled only for the purpose of relieving the tension of over-distended capillaries.

The blood has been, and still continues to be, regarded as a living fluid which carries life to all the tissues. Vital power is said to be reduced by abstraction of blood. Vital power is said to be restored or “renewed” by those remedies which increase the quantity of blood or improve its quality. That the blood is the medium by which nutrient matter is distributed to all the tissues of the body, is beyond question. That its qualities are altered in disease, and that in many instances the



blood is, as it were, the starting point of certain morbid changes, is undoubtedly correct. But that the blood transmits *vital power* to the body generally, to the tissues of the body individually—that vital power is diminished by its abstraction, or increased by any alteration occurring in the blood—seems to me utterly untenable. There is no reason whatever for assuming that what we call *vital power* can be carried from one place to another by any fluid or solid, and distributed to structures in a distant part of the body. Nor is it probable that this wonderful vital power can be added to or taken from any tissue at all. When we give stimulants, we do not increase *vital power* at all. The blood will, I dare say, continue to be called a *vital fluid*, but the term is not correct; for there is every reason to think that the soluble albuminous constituents and the red blood-corpuscles are as inanimate while circulating in the vessels of the living body as they are after the blood has been withdrawn from the vessels.

Every kind of pabulum and every stimulant, like every narcotic, and every medicinal remedy is inanimate. Everything which contributes to nutrition is lifeless. Living matter never lives upon *living matter*. The pabulum of the tissues, especially in the case of man and the higher animals, results, no doubt, entirely from the death of living matter; but, as pabulum, it is inanimate. But although the blood is not a living fluid, it contains masses of living matter (colourless corpuscles and minute particles of bioplasm in great number), and many corpuscles which are not living (fully formed red corpuscles).

Neither blood, nor lymph, nor chyle, nor cod-liver oil, nor alcohol, nor any nutritive substance whatever, can be correctly spoken of as *life-giving*. Nor do certain conditions call forth "*vital energy*," or act as "*vital stimuli*," or increase vital power which already exists. Heat, various external conditions, and excitants or irritants, as they are termed, act simply by diminishing to some extent the restrictions under which life is ordinarily carried on, and thus pabulum is permitted to come more readily and more quickly into contact with matter that already lives.

*What happens in Nutrition.*—The food that is absorbed in an altered state by the blood does not transmit life to the tissues; for, as I have shown, every tissue contains in its substance matter in a state of active vitality, or *bioplasm*. The pabulum that passes from the vessels and permeates the lifeless tissue is inanimate. It comes into contact with the living matter; and certain of its constituents then acquire at once vital properties, powers, or endowments. By contact with and absorption into the living, life is communicated to that which was lifeless. There is no reason to suppose that living matter (for

instance, the so-called "nucleus" of a cell) exerts any influence upon matter at a distance from it; but it appears probable that in all cases the changes which occur in nutrition are simply these:

The inanimate pabulum permeates the inanimate tissue (cell-wall, intercellular substance), and comes into contact with the living or germinal matter or bioplasm. Certain of the inanimate constituents of the pabulum become bioplasm. Particles of the living matter or bioplasm after a time undergo change—in fact die, and become gradually converted into inanimate "cell-contents," "cell-wall," or "intercellular substance." These inanimate *formed substances* may accumulate and undergo condensation and other physical and chemical changes, or they may be resolved by the action of water, oxygen, and the like, into new substances as fast as they are produced. Such new matters produced by the agency of the bioplasm of "gland cells" may pass into a tube or duct, and in many cases constitute a secretion.

The rate at which bioplasm grows and is reproduced is determined solely by the facility of access of the proper pabulum; so that, if nutrient matter comes into contact with the living matter readily, the living matter increases rapidly. In short, the more it is fed, the faster it grows. The *power* of growth on the part of living matter remains the same, but actual growth always occurs under certain restrictions. The restrictions or impediments to the access of nutrient matter vary in different cells, and in the same cell at different periods of its existence. Alcohol and many other substances cause changes which result in increased impediment of access of nutrient matter.

*Alcohol is not a Food, but is absorbed as Alcohol.*—It is probable that whenever alcohol is taken, a certain proportion, which increases as the dose is augmented, passes into the blood and circulates with that fluid as alcohol. It may permeate the tissues and produce changes in them. Many other active agents are taken up by the blood, and under certain circumstances produce a direct action upon the tissues. These substances may then undergo change, and perhaps be completely decomposed, and their elements excreted in an altogether different state of combination from the body. Being transmitted by the blood to different parts of the body, such soluble materials may modify the growth of tissues and affect the rate at which growth takes place. Nay, we know that by certain soluble matters introduced from without circulating with the blood the formation and growth of tissues may be actually prevented. For the further elucidation of the matter under consideration, it is desirable to refer briefly to the varying rate of growth of bioplasm under altered conditions of life.



*Of Slow and Rapid Growth.*—We can tell at once, by the characters of a cell, whether it has been growing quickly or slowly. Whenever the outer part of the cell (formed material) is firm and hard, and not very permeable to nutrient matter, the growth must have been slow. Where, on the other hand, the envelope is very thin, or where there is no envelope or cell-wall at all, the greatest facilities for rapid growth exist. Contrast the pus-corpuscle, consisting almost entirely of bioplasm, which has grown perhaps in the course of a few hours, with a fully formed cuticular cell in which the bioplasm is surrounded by a very thick layer of slightly permeable cuticular substance. The restrictions under which the growth of the bioplasm of the last is carried on are far greater than those which limit the growth of the first. But anything which renders the wall of a slow-growing cell more permeable will facilitate the access of pabulum, and its bioplasm will then increase more rapidly. Thus rapidly growing bioplasm may spring directly from bioplasm which has been growing very slowly. Pus may result from the rapid growth of the bioplasm of epithelium, fibrous tissue, nerve, or other tissue which has been growing very slowly. Cancer grows faster than healthy epithelium or other normal tissue, but not so fast as pus. Epithelial cancer is less permanent as a tissue, and grows more quickly than the normal epithelium, but it is more lasting than pus and less quickly produced. On the other hand, anything which tends to coagulate or harden bioplasm on its surface will have the effect of retarding its growth.

*Disease is not due to Defective Action.*—From the considerations advanced in the last few paragraphs it must be clear that the doctrine that disease is a *deficiency of action* must be erroneous. The idea that support is required to counteract this tendency to depression of the vital powers is purely fanciful, as is also the notion that something extra must be added in order to make up for the loss occasioned by the diseased state. Although we see structures in disease growing so fast that difference in bulk is perceptible from day to day, still pathologists continue to talk of “*deficiency of action*,” “*defective vital power*,” “*diminished vitality*,” as if it was certain that by these phrases the phenomena of disease were to be explained. The surgeon “*stimulates*” the wound with caustics, and fancies that he “*increases*” the “*vitality*” of the surface just below; the physician pours in brandy, and supposes that he “*increases the vitality*” of the affected tissues. But it is easy to prove that by these measures many cells that were alive are *killed*, and that those that escape death live and *grow more slowly than before*. This *diminished rate of growth and life* is really what is required—it is indeed the very condition which approaches to



the healthy state; but then it is, as compared with the morbid state, the very reverse of "*increased vitality*."

Probably even now most physicians would affirm that there was deficiency of vital power in a case of low pneumonia; and yet what evidence is there of such deficiency? It is true the patient is weak and cannot move; he may be delirious; all his muscles may be relaxed; his heart's action may be weak; and he may be dying of exhaustion; but has it been shown that weakness, or inability to move, or delirium, or relaxed muscles, or weak heart's action, or what we call "exhaustion," are due to diminished vitality, to depression of *vital power*? It is very well to say that in this particular morbid change there is "excess of action," and in that one there is "deficiency," or in all disease there is deficiency of action; in this condition "the vital powers are depressed, and we must give support," and in another "the vital powers are too active, and must be restrained;" but these are, after all, but arbitrary phrases, and the same words are used in very different senses.

Surely, then, it will be more correct to say that a patient is low or weak than to say that he is suffering from a depressed state of the *vital powers*. The former phrase asserts a fact; the latter expresses a theory. We talk of excess of action, and diminished action, before we have agreed as to what we mean by the terms. If we attribute rapid growth, rapid change, to increased vital activity, then most unquestionably do many diseases which have been accounted for by some supposed diminution of vital power really depend upon an undue manifestation of vital action.

*Conclusions.*—It will be observed that in these conclusions I differ materially, and in fundamental principles, from the views generally entertained. Men may agree as regards practice, although they differ materially in opinion as to why certain measures ought to be employed in a given case. But when good practice rests upon unsound theory there is always great danger of the practice being abandoned when the theory is overthrown. This in part explains why we vacillate from one extreme of practice to the other, and why, to some extent, we seem, to work in recurring circles. Earnest men not unfrequently are found to be sceptics upon the question of the proper management of disease; and, in the course of years, most valuable practical conclusions, arrived at from actual experience, are forgotten, because the theory upon which these conclusions were based has been overthrown and proved to be unsound. But why, for example, may we not retain in practice the depleting process in those cases in which it has been proved to be of service, although the principles upon which depletion

was carried to foolish extremes have been proved to be erroneous? And, on the other hand, why may we not continue to carry on the system of stimulation in cases in which stimulants are known to be useful, although it may be quite true that stimulants neither support life, nor give life, nor nourish tissues, nor supply the place of food, nor directly affect the disintegration of tissues?

So far from the abstraction of blood diminishing the nutrition of bioplasm, the process may actually be increased by violent bleeding. The blood that remains becomes weaker, and the watery parts necessarily permeate tissues more readily. Thus rapidly-growing cells, such as exist in the air-cells in pneumonia, grow still faster, because they are supplied more freely with nutrient pabulum. On the other hand, alcohol in several ways interferes with the growth of these bioplasts, and thus tends to put a stop to the "inflammatory process." The results of practice, in fact, support *this* theory: that in low conditions of the system, and by profuse bleedings, the growth of adventitious products is *accelerated*; and that such growth is retarded by alcohol, acids, and some other substances.

*Of Support.*—Further, as regards the meaning of the term "support," it is necessary to make a few observations. In cases of exhausting disease, we all talk very freely in these days of the importance of *support*; and many physicians would regard alcohol as the most valuable of all kinds of support given. Nevertheless, in these very cases the patient, in spite of all the support, loses many pounds in weight, and in the course of a few weeks or even days; nor is it possible by any known means to prevent this result. And it is a fact that, in many of the worst cases I have seen, although the stomach seemed to do its work perfectly, and the quantities of supporting matter consumed by the patient were very great, the emaciation was extreme. The patient under these circumstances has been kept alive long enough to become extremely emaciated. Such emaciation would not exist if the patient were left to nature, because death would have occurred before matters had proceeded to this extremity. But it does not follow that he has been kept alive by food or by matters that support like food. In these cases, I feel confident that the stimulant is really the agent which has kept the patient alive; for it sometimes happens that patients will not take any form of nutriment, and not unfrequently the stomach will bear whisky or brandy, and in large quantity, where it instantly rejects beef-tea, milk, and other "nutritious substances." From what I have observed, I think it possible that a patient suffering from low pneumonia, or from a very severe form of continued fever, or acute rheumatism complicated with



pericarditis and pneumonia, might be kept alive until the disease subsided by alcohol alone, but not because the alcohol acts like food and supports him and nourishes his tissues. It is certain that the alcohol does not nourish the tissues in the ordinary sense of the word; and if it diminishes the waste of the tissues in these cases, it must be admitted that it is difficult to conceive waste more extreme than that which has taken place while the patient has been taking large quantities of alcohol. But the patient lives; and so many account for the result by concluding at once that the alcohol must be a "*supporter of life*," although they are aware that this same alcohol, administered in the very same quantities in the healthy state, might destroy life.

There is not a more important question in medicine to be determined than the action of alcohol in disease; for, while it has been conclusively proved that it is not a food and does not directly nourish the tissues, there cannot be the slightest difference of opinion amongst practical men concerning its value as a remedy. We differ widely in our views as to the extent to which alcoholic treatment should be carried in a given case, but almost all agree that in some cases alcohol ought to be prescribed, and in considerable quantities.

*Chemical Action of Alcohol.*—It seems to me that we should not ignore the probable chemical action of alcohol upon important constituents of the blood. It is scarcely possible to believe that the large quantity of alcohol taken by many patients does not influence the permeating properties of the fluid part of the blood, and cause some chemical alteration in the soluble constituents which belong to the albumen class, besides exerting a local action upon soft and rapidly growing bioplasts. Many of the so-called *tonics* have the property of coagulating albuminous fluids and solutions of extractive matters. Preparations containing tannin, the mineral salts—such as the sulphate and sesquichloride of iron, nitric and hydrochloric acids, and a host of other substances employed as remedies, that will occur to everyone—possess this property, and render solutions containing albuminous and allied matters less permeable, perhaps by increasing their viscosity. The favourable action of such remedies may be due to their direct influence on the fluid constituents of the blood. They, no doubt, also cause a reduction in the rate at which blood corpuscles are destroyed, and at the same time they tend to render the walls of the bloodvessels less permeable to fluids.

But, of all the remedies we possess, I believe alcohol acts most rapidly in this way, and in particular cases most efficiently. The properties alcohol possesses of hardening animal tissues and of coagulating albuminous fluids are well known; and these



properties must not be forgotten when its effects in the animal body are discussed. Of course, when absorbed by the blood of a living person, alcohol does not actually *coagulate* the albuminous matter; but it probably renders it less fluid, and reduces its permeating property. Alcohol interferes with the disintegration of blood corpuscles; and in cases where this is going on very rapidly, and where fluid is passing through the vessels in considerable quantity, in consequence of the thin vascular walls themselves being stretched and rendered too readily permeable to fluids, alcohol is likely to be of service; but where these changes are proceeding very rapidly, and the patient's strength is fast ebbing, it may save life.

We may therefore explain the beneficial action of alcohol without assuming that it is a food, and contributes directly to the ordinary process of nutrition. Nay, if it merely filtered through the bloodvessels, and left the body by different emunctories as fast as it was introduced, we could account reasonably for the good effects we frequently observe when alcohol is given in certain forms of disease.

*Action of Alcohol upon Growing Bioplasm.*—If there be a little abrasion of the cuticle, around which the skin looks red and angry, the neighbouring tissue being hot, swollen, and painful, the capillaries distended so as to produce bright redness, it will be found that the occasional application of a drop of alcohol to the affected part will in the course of a single hour produce great changes. In and around such a spot, it is quite clear that we have not *diminished*, but *increased* action. Numerous small particles of living bioplasm ("cells") are receiving an unusual quantity of soluble nutrient substances, and are in consequence multiplying rapidly in the deep layers of the cuticle. The bioplasts of the nerves, capillaries, and connective tissue of the cutis are larger than they were in the healthy state; the living matter is growing, dividing and subdividing into smaller portions, which will grow and again divide and subdivide. In the capillaries, and just external to them, are numerous white blood-corpuscles, varying in size from small points to particles having the ordinary dimensions of these bodies. These, like the living bioplasts of the tissue, are rapidly increasing in size. The capillaries are gorged with blood, and the thin walls of many of them are stretched to the utmost. Now, what happens when a drop of alcohol is applied to such a sore? Momentary pain, followed in the course of a few minutes by great relief, or complete cessation of pain, and diminished vascularity. But how does the alcohol bring about these striking changes? If alcohol be added to any serous fluid, as is well known, the albumen is precipitated. If delicate masses of bioplasm are placed in alcohol, and afterwards examined under

the microscope, everyone knows they will appear "very granular," and they will have become shrunken and altered much in form; and it will be found that they will resist disintegration by pressure to a greater extent than in their natural state. By the action of alcohol the surface of a wound is much altered, and it soon becomes covered with a dry crust. This results from the hardening effects of the alcohol. Some of the rapidly growing particles of bioplasm are quite destroyed, while others become surrounded with an envelope of hardened matter, which prevents the possibility of their absorbing nutriment and giving rise to new particles, and growing and multiplying as rapidly as before. Not only so, but the permeating power of the nutrient fluid itself is reduced by the tendency of the alcohol to coagulate it. The most superficial of the particles of bioplasm would be destroyed by alcohol, though not so quickly, perhaps, as they would have been by the actual cautery, nitrate of silver, sesquichloride of iron, sulphate of copper, &c.

*Of the Action of Alcohol on the Vessels of an Inflamed Part.*—Next comes a more difficult question for consideration: How does the alcohol cause the vessels of the inflamed part to contract, and permit the flow of less blood through them? If we press upon the distended vessels of an inflamed part, as is well known, the blood is driven out of them, and the skin becomes quite pale; but the moment the pressure is withdrawn, the redness recurs, and exhibits *precisely the same tint* as before. From this it is clear, not only that the capillaries are unduly distended, but that the calibre of the small arteries through which the blood is distributed to them is much larger than in the normal state. Besides this, we are able to prove by this simple experiment that the vessels may be maintained for a long time of a given calibre. Such a state of things can only result from a certain regulated but definite influence upon nerve-centres which govern the calibre of the small arteries. The temporary change induced in the nerves can be maintained at one definite degree for a considerable time. In this way the quantity of blood permitted to flow through the capillaries in a given time is regulated and varied within certain limits. The mechanism is such that a small artery is made to assume and to retain for a longer or shorter period a different calibre, although this may be momentarily altered by artificial means.

Now, I believe that the alcohol passes through the walls of the capillaries, and acts directly upon the nerves, which I have shown are distributed just external to them. By these *afferent* nerves the nerve-centres are influenced, and in consequence the intensity of the current transmitted by the vaso-motor nerves of the small arteries is modified. That alcohol affects the ner-



vous system very readily is proved by its remarkable effects upon healthy persons. Can we then believe that in extreme cases of low disease, in which the nervous phenomena appear almost as in the healthy state, while the patient takes more than enough alcohol to intoxicate a healthy man, that the alcohol exerts no influence whatever? True, that in many cases, so little is the ordinary action of alcohol manifested, a patient may be taking an ounce of brandy every hour, and a bystander would not believe he was taking alcohol at all. Yet surely no one would maintain that therefore the alcohol is powerless and produces no more effect than so much water or any perfectly inert substance. That alcohol will produce delirium in health, and remove or prevent the occurrence of delirium in an exhausted state of the system, are facts, but they cannot be fully explained in the present imperfect state of our knowledge concerning the action of nerve-centres and nerves, especially the nerve-centres which control vascular phenomena.

*Action of Alcohol upon Bioplasm outside Vessels in Pneumonia.*  
—I propose now to consider what is actually going on in pneumonia, and shall endeavour to account in some measure for the beneficial action of alcohol in bad cases of this disease. The air-cells of the lung are filled with multitudes of living actively growing particles of bioplasm which have passed through the vascular wall with the serum, and are now absorbing nutrient pabulum at a very rapid rate; and probably, as these particles increase in number, an increased proportion of pabulum is diverted from all parts of the body to the “focus of inflammation,” where they are multiplying. We know positively that a determination of common salt does actually take place to this spot; and it is, therefore, perfectly reasonable to infer that other matters are in the same way diverted from their usual destination, and absorbed here, instead of being used up in the ordinary change occurring in the normal state. (Med.-Chir. Trans. vol. xxxv., 1853.) I have already spoken of the pabulum which in a fluid state transudes through the stretched walls of the bloodvessels, and feeds the particles of bioplasm which in inflammation lie just outside them. Not only are the capillaries more readily permeated by reason of the stretching and consequent thinning of their walls, but the fluid in the bloodvessels possesses a greater tendency to permeate animal membrane; so that it would seem but reasonable to consider whether anything can be done to diminish this by altering the character of the fluid itself.

Let us now consider how alcohol acts upon the elementary parts of the living organism; and in order to render the arguments upon which my views are based intelligible, it will be as well to direct the reader's attention in this place to some points



which may have been already referred to more than once in these lectures.

In the treatment of a sore upon the surface of the body, the means we adopt in order to promote the healing process check the free and rapid growth of the bioplasm. The commencement of the healing process is associated with more slow nutrient change. In the same manner what we desire to effect in the treatment of internal inflammations and the feverish state, is to check the undue growth of bioplasm which is proceeding in the blood in certain parts of the body, and in the "inflamed" tissues or organs. This bioplasm can only be reached through the blood. And it can be shown that the remedies which have been proved by experience to be of value in the treatment of such cases possess the property of checking the redundant growth of bioplasm, and can be taken up by the blood and distributed through the body without losing their properties. Now, in many extensive internal inflammations, such as pneumonia and pericarditis, it seems to me probable that the alcohol taken up by the blood really acts directly upon the morbid bioplasm growing and multiplying so very rapidly just outside the capillary vessels, and checks the process. It will be asked, upon the theory I have ventured to propound, How is it that the alcohol acts upon the morbid and not upon the healthy structure? But this question is not difficult to answer. The healthy cells, being surrounded by a thick, protective covering, or embedded in a matrix (cell-wall, inter-cellular substance), are not affected by it; while the morbid cells, growing so fast that time is not allowed for the production of a hard external envelope, are fully exposed to the free action of soluble matters which transude from the blood. These, therefore, undergo the changes already described, and are caused to increase more slowly, while many are destroyed. In the growing cells in the air-cells of a hepatised lung, then, there is no evidence of *deficiency of vital power*; and the remedies which act favourably really seem to act not by *increasing vital power*, but by *diminishing the rate at which vital changes are proceeding*—in fact, by causing particles of bioplasm which were living *too fast* to live *more slowly*, and by causing the *death* of many of them.

*Author's View supported by many General Facts.*—This explanation of the action of alcohol is in harmony with many broad facts familiar to all. By it we may account for the shrivelling of the hepatic cells, the shrinking of the secreting structure, and the increased hardness and condensation of the entire liver, which result from the continual bathing of the gland-structure by blood almost constantly loaded with alcoholic poison. It explains the gradual shrinking and con-

condensation of tissues which occur in persons who have long been accustomed to excess. The tendency to increased formation of adipose tissue which occurs in many of those who live generously, and seems to be augmented by alcohol, may be explained upon the same view, and the stunting in growth which follows its exhibition to young animals is also accounted for.

*Action of Alcohol upon the production of Heat:*—In a slight catarrh in a healthy man, six or eight ounces of alcohol did not lower the temperature, which however was only 100·7 (Parkes and Wollowicz). This fact cannot be advanced as an argument against the value of alcohol in cases of fever and internal inflammation. In slight febrile disturbances there is no doubt whatever not only that alcohol in large quantities does no good, but that it does positive harm, by exciting the heart's action unduly and tending to produce narcotism. In very low states of system, however, albuminoid matters are fast escaping from the blood, the blood corpuscles are undergoing rapid disintegration, and the living matter or bioplasm of the vessels of the neighbouring tissues and of the blood is growing very quickly. The changes referred to are associated with a rise in bodily temperature. Alcohol, as has been shown, tends to modify all these phenomena. And I believe that by further research in the same direction we shall succeed in giving a full and thoroughly satisfactory explanation of the action of alcohol in lowering the body heat in severe attacks of fever.

*Summary.*—The observations upon the action of alcohol on the “cells” or elementary units of the tissues justify us in concluding that its beneficial influence in very bad cases of disease is due in part to its action upon the pabulum and its tendency to render albuminous solutions less permeable, and partly to its direct action upon particles of naked and living bioplasm. Alcohol reduces the permeating tendency of the serum; it checks the disintegration of blood corpuscles; it prevents the rapid growth of living matter; and it interferes with or modifies chemical changes taking place in organic fluids. When these changes are proceeding very rapidly, the capillary circulation beginning to fail, the heart's action becoming very weak and fluttering, and the strength ebbing fast, alcohol may save life.

In conclusion, the local and general action of alcohol may be shortly summed up as follows:—

1. In external wounds, and in internal diseases where alcohol acts beneficially, the good result is in part at least due to the alcohol checking the *increased action* already established.



2. Alcohol does not act as a food; it does not nourish tissues. It may diminish waste by altering the consistence and chemical properties of fluids and solids. It cuts short the life of rapidly growing bioplasm, or causes it to live more slowly; and thus tends to cause a diseased texture, in which vital changes are abnormally active, to return to *its normal and much less active condition*.

3. In "exhausting" diseases, alcohol seems to act partly by diminishing very rapidly the abnormally increased growth of bioplasm. The quantity required will depend upon the extent to which the changes alluded to have proceeded. In extreme cases half an ounce of brandy, or even more, may be given for a time (in some cases even for several days) every half-hour; and there is reason to believe that in desperate cases life is sometimes saved by this treatment.

*Practical Conclusions.*—Lastly, I shall venture to repeat here the conclusions I arrived at many years ago concerning the great value of the alcoholic treatment of low fevers and inflammations. Increased experience has afforded further confirmation of the correctness of the statements made in the paragraphs below. I do not, of course, refer to slight cases of fever, pneumonia, &c., in which no stimulant whatever may be required, but to *very severe cases of disease only*.

1. In what appeared hopeless cases, as much brandy as the patient could be made to swallow (an ounce and a half to two ounces in an hour) has been given for several hours in succession, and then as much as thirty ounces a day for several days, not only without producing the slightest intoxication, vomiting, or headache, but the treatment has been followed by recovery.

2. I would adduce the fact that a man not accustomed to drink, when suffering from acute rheumatism, complicated with pericarditis with effusion, pneumonia at the base of one lung, and pleurisy on the opposite side, has taken twenty-four ounces of brandy a day for eleven days; the tongue being moist and the mind calm during the whole time. While under this treatment, inflammatory products were absorbed, and the general state of the patient much improved.

3. I have been compelled to give a very weak child, weighing less than four stone, twelve ounces of brandy a day for ten days, while suffering from acute rheumatism with pericarditis and effusion. This quantity did not produce the slightest tendency to intoxication, or exert other than a favourable effect upon the disease. The patient did not begin to improve until the quantity of brandy, gradually increased, had reached the amount stated.



4. I would state that among the general conclusions I have arrived at, after carefully watching more than one hundred serious cases of acute disease treated with large quantities of stimulants, are the following:—That intoxication is not produced; that delirium, if it has occurred, ceases, or is prevented from occurring at all in the course of the case; that headache is not occasioned; that the action of the skin, kidneys, and bowels goes on freely; that the tongue remains moist, or, if dry and brown, often becomes moist; that the pulse falls in frequency and increases in power; that respiration is not impeded, but that, where even one entire lung is hepatised, the distress of breathing is not increased, and it appears that the respiratory changes go on under the disadvantageous circumstances present as well as if no alcohol had been given.

The conclusion from all this is, most certainly, that alcohol does not do harm in fevers and acute inflammations; that it does not produce intoxication in persons suffering from exhausting diseases; and that large quantities (from twelve to thirty ounces) may be given in cases which appear very unlikely to recover, and sometimes the patient will be saved. The conviction is forced upon the observer that, in desperate cases, these large quantities of alcohol are directly instrumental in saving life, not by *exciting or stimulating to increased action*, but by *moderating actions already excessive*, and at the same time by causing the heart to contract more vigorously, and so continue to drive the blood through the impeded capillaries.—*Medical Times and Gazette*, May 25, July 13 and 27, 1872, pp. 591, 29, 88.

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#### 5.—ON THE TREATMENT OF HYPERPYREXIA BY THE EXTERNAL APPLICATION OF COLD.

In a very interesting and exhaustive paper upon this subject, Dr. STRAUS of Strasbourg goes over the entire history of the treatment of hyperpyrexia by the external application of cold, from the time of Hippocrates down to the present day, and the conclusions he arrives at are as follow:—If a healthy man be placed in a cold bath he speedily becomes chilled; his skin pale, *cutis anserina*; he shivers, and the thermometer in the axilla falls in proportion to the coldness of the water employed; but if the temperature of the interior of the body be taken in the anus, this is found to be normal, or only to fall a few tenths of a degree, and it only falls in any important degree when the experiment has been pushed to an extent which dare not be approached in therapeutics; but then the innervation of the heart and lungs becomes seriously implicated, and the bath cannot be prolonged without compromising life itself. A sound

man therefore, plunged into cold water, maintains the mean normal temperature of his blood notwithstanding the increased abstraction of heat from his surface, and he does so by an increased production of heat, which persists for a time, and is revealed when he is removed from the bath, dried, and clothed, by the production of that transitory febrile state termed the reaction, which is accompanied by a cutaneous temperature above the normal. Similar results occur in pathological conditions: a fever patient does not merely retain more heat than a healthy man, he produces it; instead of  $37^{\circ}\text{C.}$ , his normal temperature is  $39^{\circ}\text{C.}$  or higher, and if he be plunged into a cold bath the results are similar. The cutaneous temperature is speedily brought down to that of the bath; the anal temperature falls only in a relatively insignificant amount, never more than one degree; and when removed from the bath the reaction occurs as in the physiological condition, so that at the end of two or three hours at the most, the initial temperature is attained or even surpassed. Even supposing, then, that this trifling and temporary lowering of the temperature, of a maximum of one degree, and lasting at the longest for two or three hours, should be considered desirable of attainment, it would be necessary to give the patient from ten to fifteen baths in the twenty-four hours, involving a most laborious practice for the attainment of a most trifling result.

There is one point, however, remarks Straus, upon which all the partisans of cold are agreed, and that is the sedative influence which the cold bath exercises upon the affections of the nervous system, and that this use of cold is most efficacious in delirium, cerebral affections, cephalalgia, singing in the ears, &c., and that it has thus a most favourable action in the malignant forms of fever, especially in the ataxic and adynamic forms. But, adds Straus, similar results have been obtained with much more certainty and energy by Currie's old method of cold affusion, which is more especially of the greatest value, and is specially indicated in an accident of extreme gravity which occasionally occurs in fevers—collapse.

It sometimes happens during the course of a fever that, when the thermometric ascension has reached its height, the temperature taken externally experiences a sudden fall from  $40^{\circ}$  or  $41^{\circ}$  to  $34^{\circ}$  or  $32^{\circ}\text{C.}$ , while the anal temperature still maintains its former height. The skin is at the same time covered with a cold and clammy sweat, the heart contracts feebly, and the pulse becomes extremely feeble and thready. Such is the condition to which Wunderlich and his disciples have given the name of collapse.

The skin is chilled because the heart no longer has the force necessary to impel the circulation through it, the heart is



paralyzed, and the lungs also, as seen in the short, superficial, often unequal and intermittent respirations, and revealed on auscultation by the gradually-increasing engorgement of the lungs and the accumulation of the bronchial secretions, which the patient has no longer power to expel, exactly as happens after section of the pneumogastric nerves. And this collapse no doubt does arise from paralysis of these nerves, or rather of those nervous centres which preside over the functions of respiration and the production of heat, and it is these centres which it is so important to rouse by reflex excitement. The readiest method of doing this is the cold douche: a waterproof sheet is slipped under the patient, his shirt is removed, and he himself raised to the sitting posture, and a pitcherful of water, at a temperature at or a little above 10° C. (50° F.), is then poured over his head from a height of from one foot and a half to three feet. The result is marvellous, and is one often, nay, almost daily, observed in the clinique of Professor Hirtz. The patient, plunged in stupor, is suddenly roused by the shock; he draws a long breath; the respiration becomes fuller, freer, and more regular; the cardiac ataxy ceases, and the pulse becomes fuller and stronger; traced by the sphygmograph during the collapse, it presented a horizontal line, hardly broken by a few feeble undulations; during the affusion the line of ascension is reproduced and becomes well marked, showing increase of cardiac force, and also of arterial tension. The affusions should be repeated during two or three minutes; when finished, the patient is dried, laid down and covered up, the waterproof being removed. The thermometer, now applied in the axilla, shows that the external chilliness has disappeared, and that the temperature there is now normal or even higher, the anal temperature being considerably reduced. The temperature has, in fact, been redistributed, and the equilibrium between the external and internal temperature restored, the rupture of which is in a measure characteristic of the condition of collapse. The lungs, too, are now found to be acting freely, the râles are diminished, and the passing congestion is disappearing.

It is easy, however, adds Straus, to understand the physiological action of this treatment; cold employed in the form of affusion—that is, quickly and temporarily—acts not by subtracting heat, for the amount actually removed is perfectly insignificant, but by exciting, by its powerful and energetic reflex action, the nervous centres which preside over and regulate the circulation and production of heat. While acknowledging, therefore, the utility of Currie's cold affusions in febrile nervous affections, delirium, ataxy and even in collapse, and also the advantage and propriety of cold sponging in



refreshing the patient, and in cleaning and exciting the skin, Straus regards the use of the cold bath in the treatment of fever as a most expensive, troublesome, and possibly dangerous mode of producing very trifling results, and as far inferior to the use of such antipyretic remedies as quinine and digitalis, adding that a single draught of one gramme (15 grs.) of digitalis would lessen the fever more than a whole series of cold baths.—*Bull. Gén. de Thérap. and Edin. Med. Journal*, June, 1872, p. 1137.

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#### 6.—THE COLD BATH IN THE TREATMENT OF ENTERIC AND TYPHUS FEVERS.

Cases under the care of Dr. ALEXANDER COLLIE, at the Homerton Fever Hospital.

We purpose giving a short account of some cases of typhus and enteric fevers treated at this hospital by the cold bath. That in some of the following cases this treatment was very useful is beyond a doubt, and in none is there any evidence that the disease was made worse by it, not even when severe lung affection co-existed. The patient in the second case seems to have been in *extremis*, and it would require very great faith in the *vis medicatrix naturæ* to believe that the patient recovered in spite of the treatment, for the beneficial effect of the bath was constant and well-marked. It is further interesting to note that in those cases which ultimately did well the immediate effects were beneficial, the temperature being lowered and sleep produced.

*Case 1* was a young woman, aged twenty-eight, admitted on October 29th, suffering from a severe attack of enteric fever complicated with bronchitis. She became convalescent on November 14th. On the 27th, however, diarrhoea came on, the respiration became hurried, and the tongue dry and fissured. On December 3rd the temperature in the rectum was 104·4° F.; pulse 132; numerous râles could be heard through both lungs; the abdomen was distended and the bowels loose, but there was no eruption. In this condition the patient was placed in a bath of a temperature of 80° F., which in fifteen minutes was lowered to 67°. In ten minutes after being put into the bath the thermometer in the rectum registered 100·4°. After being removed from the bath the patient immediately went to sleep. On December 4th the temperature in rectum was 105°, but fell considerably after a bath of 68°, lowered to 52° in fifteen minutes. On the 5th, 6th, and 7th, baths were given. On the 8th the temperature was normal, but on the 9th it had gone up to 100·2°, and the bath was repeated, when it fell again to

98·6°. On the 10th, thirteen days after the relapse, the patient was convalescent; temperature normal. She was discharged on January 30th.

*Case 2* was a young woman, aged twenty-five, admitted Dec. 5th, with but slight pyrexia, but on the 10th well-marked typhus rash was present. Temperature 103·4°; pulse 160; tongue dry and brown; well-marked carphology, restlessness, and delirium. A cold bath was thought of as a means of producing sleep; but as the patient was in a state of extreme constitutional depression, it was feared that the bath might be followed by dangerous results. She was therefore ordered a pill containing half a grain of opium, and to have cold cloths applied to the head. The opium did not produce sleep, and from this time up to the 15th, probably the twelfth day of the disease, the patient exhibited subsultus, twitchings, floccitatio, deafness, and all the signs of threatening dissolution. Pulse 132 soft; temperature 100·6°. A bath was ordered of 80°, to be cooled in twenty minutes to 60°. During the bath the patient's pulse became alarmingly feeble, and stimulants were administered. In about twenty minutes after being put into the bath the temperature of the patient fell to 97·8°. After the bath three ounces of whisky and a raw egg were given, and the patient was wrapped up in a warm blanket, and soon fell asleep. By midnight the temperature had gone up to 101·4°. On the 16th (thirteenth day of disease) the temperature was 103·4°; a bath was ordered from 77° to 54°, but this time the temperature fell only one degree, but sleep followed the bath. On the 17th the temperature was 104·8°, and, notwithstanding the presence of severe bronchitis, a bath from 90° to 60° was ordered, and the temperature fell to 96·2°, and the patient slept well. On the 18th the patient was better, the nervous twitchings were less, and the bath was not repeated. On the 20th the temperature was normal. On Jan. 29th patient was discharged.

*Case 3* was a married woman, aged twenty-eight, admitted Dec. 12th, who said that she had been ill ten weeks, but of what was not clear. A bath of 90° lowered to 52° in thirty minutes had the effect of bringing down the temperature from 103·2° to 100°. No immediate sleep followed the bath, but the patient slept well during the night. On the 14th the bath was repeated, and the temperature lowered, and the patient slept immediately after; and although by midnight the temperature had risen to 105°, the patient had a good night. On the morning of the 15th the temperature was 104·2°, but was lowered to 102·2° by a bath, but by midnight it had risen again to 105·4°. On the 16th the bath was not repeated, as the patient was sweating freely. On the evening of the 17th the temperature was 104·4°.



but fell to  $102.6^{\circ}$  after a bath. In the evening of the 18th the temperature was  $103^{\circ}$ , but the bath was not repeated. In the morning of the 19th it was  $102^{\circ}$ . From this time to the 28th the temperature ranged about  $100^{\circ}$ , when it became normal, the patient being discharged on the 31st of January.

Case 4 was a boy, aged seventeen, admitted on Dec. 17th with enteric fever. On the 22nd, after a bath of  $85^{\circ}$  to  $58^{\circ}$ , the temperature fell from  $105.2^{\circ}$  to  $102.8^{\circ}$ , but no other good result followed. On the 23rd the temperature had risen to its former height, but as the patient objected to the bath it was not insisted on. On the 24th, however, the urgency of the symptoms suggested the bath, which was accordingly given, with the effect of lowering the temperature from  $104.8^{\circ}$  to  $101.8^{\circ}$ . The patient died sixteen hours after this bath, and at the autopsy extensive peritonitis and perforation of the bowel were discovered.—*Lancet*, September 21, 1872, p. 410.

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## 7.—THE TREATMENT OF SMALL-POX BY VACCINATION.

By Dr. WILLIAM MARSHALL, Mortlake, Surrey.

[Vaccination is not only preventive but actually curative in small-pox. Dr. Marshall and Mr. Furley have evidently come to somewhat the same conclusions unknown to one another. Mr. Furley has, however, gone further than Dr. Marshall in injecting lymph into the system. Dr. Marshall's conclusions as to the effect of vaccination in arresting small-pox are, however, more fully elaborated, being the result of experiments and observations extending over a period of five or six years.]

In estimating the value of vaccination as a cure for small-pox, it is necessary to be acquainted with the course of that disease in all its varieties, and to bear in mind that its incubative stage is thirteen days; and that vaccination, as far as my observation goes, is a local disease until some time between the eighth and eleventh days; it is only then that the constitution becomes affected, and it is so from absorption of the lymph in the matured vesicles. I come to this conclusion from observing that a person exposed to the infection of small-pox within eight days after being vaccinated may take the disease; but no matter how much exposed after the eleventh day, he will not take it. Also, if a person who has been vaccinated be revaccinated within seven or eight days, the revaccination will take in some modified way; but if done after the eleventh day, not at all. Hence, if an unvaccinated person who has been exposed to the infection of small-pox be vaccinated in time for the vesicles to



rise, and the lymph to be absorbed before the symptoms of small-pox begin, he will either have no attack, or, what I think is more common, will have severe symptoms, ending with the appearance of a few spots, sometimes of none. But if the symptoms of small-pox commence before the vesicles have risen and begun to desiccate, he will have a severe attack, but modified in so far that there will be no secondary fever.

I shall not occupy space further than in giving the following history.

In a large family an unvaccinated child had small-pox and died. I only saw it when dying. I recommended all the others to be vaccinated. Four had been previously vaccinated; these were revaccinated; none of them took the disease. Three of the sons had never been vaccinated; one of them I vaccinated directly and satisfactorily, but just as the vesicles were desiccating, he showed symptoms of small-pox, and five spots appeared on his face, and a few on his body; he was not confined to bed a single day. The second on the very day he was vaccinated became ill with small-pox, and the third, who had never been vaccinated, was taken ill on the same day. Both of these had the disease severely, but the contrast between the two lying in the same room was most remarkable. In the second son the vaccination and the small-pox progressed, the one entirely unaffected by the other until the tenth day; the vaccine vesicles then desiccated and so did the small-pox ones, and the disease came to an end; he had *no secondary fever*, and was sitting up *quite well*, while his brother—the unvaccinated one—was lying in a high secondary fever, delirious, and battling for his life. These cases occurred six years ago, and this was the first instance I had seen of the curative effects of vaccine. Since then, however, I have seen many, and have always observed the same result, so that I think that I can have made no mistake, I was, therefore, much surprised a few weeks since to see in the summary of the trial of some of the “Peculiar People” for the manslaughter of two children who had died of small-pox without medical aid, &c., that a third who had been vaccinated, had small-pox directly after and died of it. I should be very much pleased if the doctor who attended the case would publish details of it, especially with regard to the date of the onset of the symptoms of small-pox after the vaccination, the date of the death, and the cause of it.

The great objection to the use of vaccination in the cure of small-pox is the necessity, in my experience, of its being done within a day or two of the onset of the disease, otherwise the vesicles have not time to rise and affect the system in a curative manner; but Mr. Furley’s plan of injecting a large quan-

tity of lymph under the skin seems to be exactly what is necessary, and may be done with hopes of immediate benefit at any stage of the disease.—*Lancet*, June 15, 1872, p. 825.

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### 8.—THE TREATMENT OF SMALL-POX BY VACCINATION AND THE INJECTION OF LYMPH.

By R. C. FURLEY, Esq., Edinburgh.

The doctrine universally held, that vaccination will be of “no use” if delayed till five days after the inhalation of the germ of variola, I think, is erroneous. For some time past I have been in the habit of vaccinating every case of small-pox that has come under my care; and the result, as I shall show, is very encouraging, and seems to indicate that vaccination is not only prophylactic but *curative*. I have found, however, that, although the ordinary process of vaccination by scratching the arm is sufficient to modify the disease in infants, it is almost inoperative in adults. After having discovered that the ordinary process was almost useless in grown-up persons, I adopted the method of injecting lymph by means of Dr. Wood’s hypodermic syringe. This instrument I also found occasionally to fail, because, on passing the wire through the needle after the operation to keep it clear, I have found it to eject the lymph which I had hoped had been in the blood. This, I am afraid, has been the cause of two otherwise unaccountable failures out of sixty cases. How to obviate this source of fallacy occupied my thoughts for a considerable time. At last, however, I succeeded in inventing an instrument that promises to transfer lymph directly from the tube in which it is contained into the circulation. It consists of a hollow needle with a bore sufficiently large to admit of the introduction of a vaccine tube. The process consists in passing the point of the needle charged with a tube of lymph under the skin, and blowing the lymph directly into the blood. I have purposely described this minutely, in order that, if any of my brethren do me the honour of verifying the results of my treatment, they may not commit the errors which experience has pointed out. It would occupy too much space to describe many cases; I shall therefore only give three to illustrate the effect of the treatment at the different periods of life—infancy, childhood, and manhood.

*Case 1.*—Baby C., aged one month, and unvaccinated. When first seen, the papular eruption was over the face, hands, and legs. There had been three cases of small-pox in the house. One had been sent to the Small-pox Hospital, where the mother had visited him. I ascertained that this was really the case from the resident physician, Dr. Saxby. I vaccinated the



child at once. Next day, when I called, the eruption had entirely disappeared, with the exception of two papules on the face, which were now more prominent. On the third day these two papules disappeared, and a fresh crop of four-and-twenty, chiefly over the head, made its appearance; and these became hard, and did not fill like ordinary vesicles. In three days these had also disappeared. The vaccination itself did not show any signs of taking till the tenth day, and was matured on the thirteenth.

*Case 2.*—M. R., aged thirteen years, vaccinated in infancy, and whose sister had died of small-pox a week before. I was called in on a Saturday, and found her face considerably swollen, and the papular eruption on the hands and forearms. I at once *injected* two tubes of lymph into the arm. On Sunday the eruption had disappeared from the hands and arms, and appeared on the feet. On Monday it had spread up the legs and trunk. On Tuesday it had entirely gone away, and there was nothing to be seen but the inflamed areola at the point where I had injected.

*Case 3.*—J. W., aged thirty-four, a stableman of dissipated habits, and never vaccinated. I saw him on the second day of the eruption, and injected two tubes of lymph. The case, instead of being confluent, as might have been expected, was discrete, except at the alæ of the nose. He went on very favourably, and the eruption began to desiccate on the ninth day instead of the fifteenth, eighteenth, or twentieth. There was no areola at the point of injection.

I think these cases, selected from a number of such, show that there is at least a germ of truth in my statement that vaccination is curative of small-pox. I find that the treatment is much more successful in the youthful than in the adult period of life; and I am beginning to think that this arises from adults requiring, as it were, more of the remedy. It is also more effectual the earlier the operation is performed. I have usually found that the vesicles did not attain the size they do when the disease is allowed to run its course; they did not often become pustular, and desiccated earlier.

No doubt I have had failures and deaths; the latter five in sixty cases, in three of which I did not look for recovery from the first on account of other complications, and two were unlooked for: all were females—a fact of itself of some significance. I reserve for a future communication a detailed summary of all my cases; meantime I trust I have said sufficient to encourage my brethren calmly to investigate the subject.

I should have mentioned that the instrument I employ for transferring the lymph directly into the blood was made for me by Mr. Young, cutler, North-bridge.—*Lancet*, May 25, 1872, p. 717.



### 9.—CASES OF SMALL-POX TREATED BY THE HYPODERMIC INJECTION OF VACCINE LYMPH.

By Dr. ROBERT GRIEVE, Medical Superintendent, Metropolitan Hospital, Hampstead.

In the *Lancet* of the 25th May, Mr. Furley, of Edinburgh, brings under the notice of the profession the alleged curative action of vaccine lymph in small-pox. My attention had been previously directed to letters sent by him to a daily newspaper in Edinburgh, in which ordinary vaccination was lauded as a specific and certain remedy for variola. Hoping fervently that Mr. Furley was in the right, and that it would be proved that my ideas about the want of any influence of cow-pox over small-pox when concurrent, unless induced before the sixth day of the period of incubation of the latter, although derived from the observation of a considerable number of cases, would turn out to be erroneous, this plan of treatment was tried on six of our patients. Three of the six died, and in the others the disease followed its usual course, uninfluenced to any noticeable extent by the vaccination. These were not very successful results, but, as with one exception the subjects upon whom the treatment was tried were adults, they agree with Mr. Furley's, for he now says that "it (vaccination) is almost inoperative in adults." Believing, apparently, that the vaccine poison is a true antidote to the variolous, and the quantity required to be introduced into the system to neutralise the disease is larger than that used in the ordinary operation of vaccination, he now recommends the hypodermic injection of the lymph. Upon this theory nothing need be said, although it seems to me that it is by their great similarity, if not virtual identity, that the vaccine acts upon the variolous disease, and that if Mr. Furley is right in his deductions, in this instance at least the dogma of homœopaths is correct, *similia similibus curantur*. But putting theory on one side in order to give the practice a full and fair test, seven patients were operated upon after Mr. Furley's method. In this communication I propose giving the details of the cases so treated as entered in the hospital case books by Mr. Bland, assistant medical officer, appending only such remarks as may be necessary to convey the impressions received while watching the daily progress of the patients.

One point which Mr. Furley's paper brings out forcibly, and it is of very general application and interest, is this: that the deductions either from statistics of mortality or from the results of any particular line of treatment in small-pox are sure to be fallacious unless the age and particular state as to primary vaccination of every patient under notice be considered. In Mr. Furley's instance, he had had by his own statement five

deaths in sixty cases. It entirely depends on the conditions mentioned above whether this is to be considered a low rate of mortality or not. If all his patients were unvaccinated it is assuredly a very low one; but if they were all between the fifth and twentieth years of age, and had four or more good marks of primary vaccination, then it is a high rate, especially for private practice. These suppositions are both extreme, but in the absence of the particulars required for making a correct estimate, any opinion formed as to the success or non-success of Mr. Furley's treatment is pure conjecture. Another difficulty in forming a correct judgment as to effects of remedies in small-pox in the *vaccinated* subject is the impossibility of telling at the earlier periods of the attack, say up to the sixth day, what will be the exact course of the disease even if left to itself. I have often seen patients who on the fourth and fifth days of the disease had a temperature of over  $104^{\circ}$ , whose faces were swollen, and of a general bright-crimson hue, and who were delirious at night, and on the eighth or ninth day they felt well, their temperature was normal, and a few hard scales was all that remained of the eruption that promised to be so severe; and this without treatment of any kind. In the *unvaccinated* class it is different: the disease may not always take the confluent form, nay, sometimes it is quite discrete, but it is very rarely indeed that the rash aborts; however sparse it may be it reaches the pustular stage, and the pustules are full-sized and umbilicated. In my experience, in all vaccinated cases the disease is modified, even when it proves fatal, and the proportion of instances is very large indeed in which it runs a highly modified and more or less abortive course. In the Hampstead Hospital the mortality in this epidemic in the vaccinated of all classes has been 11.40 per cent.; it is found that in about 12 per cent. the disease is severe, either confluent or semi-confluent, leaving over 75 per cent. for very mild and aborting cases. One can therefore imagine it possible for a practitioner to have in succession fifty or sixty mild cases, especially in a community said to be so well protected by vaccination as that of Edinburgh is.

The method followed in injecting the lymph was strictly as described by Mr. Furley, through a grooved needle widened at one extremity (made for me by Messrs. Arnold and Son). The contents of two tubes of vaccine lymph were blown into the subcutaneous tissue on the outer side of the arm, near the usual place of vaccination. The lymph used was taken from a primary vaccination in a healthy child on the eighth day of the pock, and was only five days old.

Case 1.—J. S., male, aged twenty-five, clockmaker, admitted June 2nd. Two vaccination marks. Five days ill. Rash vesi-



cular, small, and very copious; semi-confluent on face; the areola well defined.

June 3rd. Temperature  $102.8^{\circ}$ ; pulse 120. Eyes suffused; coughs a little; bowels open; takes food fairly; had a quiet night; tongue furred, red at tip. Large crepitation distinct over both lungs posteriorly. Expectoration frothy and very profuse. Respiration 24. Vaccine lymph injected (in presence of representative of the Lancet).—9 p.m.: Temperature  $101.4^{\circ}$ ; pulse 104; rash the same as this morning, but fuller.

4th. Eruption on face vesicular, small in size, very copious, and semi-confluent; on trunk and limbs also vesicular, small in size, but less copious and discrete. Lips swollen; eyes watery; tongue slightly furred and moist; cough less, and expectoration slight. Muttering delirium all night; no sleep. Bowels confined. Swelling and tenderness at seat of injection. Pulse 104; temperature  $101.6^{\circ}$ ; respiration 30. 9 p.m.: Temperature  $100.4^{\circ}$ .

5th. Eruption matured, and in some places exudation taking place from pustules on face; on trunk and limbs still vesicular. Swelling of upper lip increased; some general swelling of face; lachrymation; great tenderness of skin; tongue dry and glazed. Wandering delirium during the night; very little sleep. Eyes suffused, and conjunctivæ injected. Redness and tenderness at seat of inoculation. Respiration loud and cooing, 30. Expectoration less. Pulse 112; temperature  $101.8^{\circ}$ . 9 p.m.: Temperature  $100.6^{\circ}$ .

6th. Eruption beginning to dry on face, pustular on limbs and trunk; swelling of face slightly less. Slept well; no delirium. Tongue dry; eyes suffused, and conjunctivæ much injected; bowels open. Takes food well. Still slight swelling, tenderness, and redness at seat of inoculation. Temperature  $103.2^{\circ}$ ; pulse 130; respiration 24. 9 p.m.: temperature  $103.4^{\circ}$ .

7th. Eruption on face crusting round the mouth and nose, fully pustular on other parts; face still swollen. Has had a very restless night; no delirium. Lies in a semi-stupor, with his head thrown back. Tongue very red and dry. Respiration 30, and rather laboured; pulse 130, feeble; some tremulousness of hands; temperature  $104.6^{\circ}$ . The whole arm at seat of injection is swollen, hard, red, and painful. 9 p.m.: Temperature  $104^{\circ}$ .

8th. Eruption on face drying; on the hands the rash is becoming horny, with a few pustules here and there. The whole of the right arm is swollen, tender, inflamed, and becoming brawny to the touch. Tongue dry and brown. Had a rather severe rigor last night, lasting a quarter of an hour; quiet night; slept well. Bowels confined. Pulse 120; temperature  $103^{\circ}$ ; respiration 32. No cough or expectoration. 9 p.m.: Temperature  $103.2^{\circ}$ .



9th. Face covered with irregular crusts; on limbs and trunk nearly dry. Slept well; no delirium. Tongue dry and glazed; bowels open; very tremulous; no swelling of face; fluctuation becoming distinct over the whole of the anterior surface of right arm, more particularly at the seat of operation; forearm œdematous. Pulse 112; temperature  $101^{\circ}$ ; respiration 32, laboured. The arm was incised, when about two ounces of thin pus escaped. A probe passed into the wound went in a direction downwards and outwards for about four inches. To have sulphate of quinine mixture, half an ounce, every three hours. 9 p.m.: Temperature  $103^{\circ}$ .

10th. Eruption quite dry on face, and crusted on trunk and limbs; drying fast. The wound has discharged very freely since yesterday, quite half a pint of thin pus escaping; swelling and inflammation somewhat less. Was very delirious all last night; purged three times. Tongue dry and tremulous, and he is unable to protrude it; countenance pinched and drawn. Takes milk and beef-tea well. There is general muscular tremor. Pulse 120; temperature  $101^{\circ}$ ; respiration 32, stertorous. 9 p.m.: Temperature  $102^{\circ}$ .

11th. Arm discharging very copiously pus thin and unhealthy, otherwise the arm the same as yesterday. The patient lies on his back without moving, with mouth open, and breathing loud and deep. He is losing flesh rapidly; diarrhoea set in last night, but is now stopped; had a draught of catechu and opium. Tongue dry, brown, and tremulous. Was rambling and talking all night; no sleep. Bowels acted once to-day. Takes food well; prostration very great; sweats a great deal. Pulse 112, very compressible; respiration 34; temperature  $102.6^{\circ}$ . Both lungs posteriorly have a want of resonance; respiratory murmur very feeble; anteriorly resonance good, but murmur feeble, although less so than posteriorly. 9 p.m.: Temperature  $104.6^{\circ}$ .

12th. Pulse 140, tremulous; respiration 40; temperature  $104^{\circ}$ . Incision has sloughed to the size of a half-crown; the cellular tissue quite dissected out from between the muscles. Diarrhoea began again last night, and still continues (about six times in twelve hours); motions very loose, and passed under him. Delirious rambling and talking all night. Tongue very dry, brown, and tremulous; has a general tremor; weakness very much increased; sweats very much; skin quite moist and clammy. Takes food very freely. Died at 5.40 p.m., after convulsions.

Permission for a post-mortem examination was refused. This was a severe case of small-pox, complicated in the first place with bronchitis, and afterwards by consequences which were clearly traceable to the injection of the lymph. It was found, on inquiry into his previous history from his friends, that he

had not enjoyed good health for some time, and had suffered from some chest affection.

*Case 2.*—E. L., female, aged twenty-two, domestic servant, admitted on the 3rd of June, on the fourth day of her illness, with a papular rash, small in size, moderately copious in quantity, and discrete on the face; on trunk and limbs the eruption is sparse; temperature  $99^{\circ}8'$ . In the afternoon the lymph was injected in the usual manner (also in the presence of the representative of the Lancet). 9 p.m.: Has had a comfortable day; tongue furred; eyes suffused; takes food freely; pulse 88; temperature  $101^{\circ}4'$ .

June 4th. Eruption small and becoming vesicular; no swelling; had a quiet night, but without sleeping; slight soreness of throat; temperature  $99^{\circ}4'$ ; pulse 88. 9 p.m.: Temperature  $103^{\circ}$ .

5th. Eruption maturing irregularly on face, where there are a few pustules, with a large number of vesicles and papules hard to the touch; on the trunk and limbs eruption small and aborting. Slept well: no swelling or uneasiness, except at the seat of the injection, where there is slight enlargement and tenderness; pulse 104; temperature  $100^{\circ}$ . 9 p.m.: Temperature  $101^{\circ}2'$ .

6th. Pustules drying in places; pulse 96; temperature  $99^{\circ}8'$ . 9 p.m.: Temperature  $100^{\circ}$ .

7th. Eruption generally drying and hardening; sleeps well; tongue clean and moist; the pain and swelling at the place of injection has quite gone; pulse 90; temperature  $99^{\circ}4'$ . 9 p.m.: Temperature  $99^{\circ}$ .

8th. Pulse 84; temperature  $98^{\circ}6'$ . 9 p.m.: Temperature  $98^{\circ}5'$ .

9th. Eruption quite dry; small crusts left, which are beginning to fall off; pulse 96; temperature  $98^{\circ}5'$ . 9 p.m.: Temperature  $98^{\circ}5'$ .

10th. Face almost clear; feels quite well; pulse 72; temperature  $98^{\circ}6'$ . 9 p.m.: Temperature  $98^{\circ}6'$ .

11th. (Twelfth day of illness). Pulse 96; temperature  $100^{\circ}4'$ . 9 p.m.: Temperature  $100^{\circ}$ .

12th. Temperature  $98^{\circ}5'$ . 9 p.m.: Temperature  $98^{\circ}5'$ .

This case was a mild one from first to last, as shown by her condition when admitted. The impression of those watching her was, that the patient did not progress so rapidly as might have been expected from the general symptoms. The disease did not completely abort, or we should not have had the rise in temperature on the twelfth day, for which no accidental cause could be discovered.

*Case 3.*—M. A. P., female, aged twenty-three, domestic servant, was admitted on the 5th of June. Had three good



vaccination marks. She states that after premonitory symptoms, which she describes as severe, a rash appeared yesterday. On admission the face was thickly covered with a rash of a dusky-red colour, and slightly elevated above the surface. An eruption of the same character, but more sparse, exists on the trunk and limbs. Eyes suffused; lips swollen, and the cuticle separating, 9 p.m.: Temperature  $104.2^{\circ}$ .

6th. The eruption much the same as yesterday; lips and eyelids much swollen. Did not sleep in the night; was not delirious. Early this morning hemorrhage from the vagina set in; has slightly the characteristic hemorrhagic fetor; bowels open; tongue much coated. Pulse 140; respiration 28; temperature  $104.2^{\circ}$ . Vaccine lymph injected. 9 p.m.: Pulse 140; temperature  $104^{\circ}$ ; vaginal hemorrhage much more severe. To have ten minims of solution of morphia with fifteen minims of dilute sulphuric acid in water, every three hours; ice and iced drinks *ad libitum*.

7th. Eruption on face becoming irregularly vesicular, and seems confluent; on trunk and limbs also partially vesicular, but discrete. Swelling of the face more marked; eyes suffused; lips black; fetor stronger. Vaginal hemorrhage increased. Did not sleep and was delirious in the night; is now rational. Tongue covered with a thick brown fur. Pulse 120, small; temperature  $102.8^{\circ}$ . A patch of ecchymosis, about the size of a florin, exists at the place where the lymph was injected. She coughs slightly, and expectorates mucus streaked with blood. 9 p.m.: Temperature  $103.4^{\circ}$ .

8th. Eruption on the face of a dingy-white colour, interspersed with livid patches; on the trunk and limbs many of the vesicles are filled with a black fluid. Lips and lower part of face much swollen. Did not sleep last night, but was not delirious. Vaginal hemorrhage still severe. Tongue covered with a thick brown fur, and dry in the middle. Cough becoming troublesome, and expectoration of the same character as yesterday. She now complains of pain and a feeling of oppression across the upper part of the chest. There is dulness on percussion posteriorly on both sides, more marked on the right; loud râles are audible in the same parts. Pulse 140, very feeble; temperature  $103^{\circ}$ ; respiration 28, laboured. 9 p.m.: Temperature  $103.6^{\circ}$ . Died at 11 p.m.; was conscious to the last.

Could not obtain leave to make a post-mortem examination. This was a case of malignant small-pox, in which the hemorrhagic symptoms, although rather late of being developed, ran a short and severe course, uncontrolled by the injection of the lymph. Death occurred on the eighth day of the dis-



ease, evidently hastened by the most common complication of variola nigra—if it can be called a complication—congestion of the lungs. It was ascertained that this patient had led a rather irregular life.—*Lancet*, June 29, 1872, p. 889.

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#### 10.—A NEW THEORY OF GOUT.

By Dr. AUSTIN MELDON, Surgeon to Jervis Street Hospital, Dublin.

A glance at the history of gout will convince the most incredulous that little progress has been made in the pathology of the disease. There is, moreover, some reason to believe that in its treatment we are not much in advance of the ancients. The humoral theory of Hippocrates and Galen does not differ essentially from the uric-acid and urate-of-soda doctrine of the present day; and there is much uncertainty as to whether our forefathers did not know of the existence of colchicum or some kindred plant. Be this as it may, one thing is certain—gout is now a more common and a more fatal disease than heretofore. Within the last twenty years, at least six thousand persons have died in the United Kingdom directly from this disease, and ten times that number have perished indirectly from it. I therefore deem no apology necessary for laying my views of the nature and treatment of this important affection before my professional brethren, believing it to be the duty of every medical man to forward as far as he can the pathology of disease.

Cullen, who believed gout to be an affection of the nervous system, had few followers; it was difficult to explain by this theory many of its phenomena, and it soon, therefore, passed into oblivion. It was replaced by the reappearance of the humoral theory, which lasted until Dr. Garrod's experiments proved the existence of uric acid in the blood of the gouty. The late Dr. Gairdner believed that the disease was due to venous congestion, but with the exception of a few of his followers, here and in Germany, most medical men have adopted the uric-acid theory.

True, many do not believe, with Dr. Garrod, that the kidney is the organ whose functional impairment causes this undue accumulation of uric acid, but refer it to derangement of the stomach, liver, or kidneys; the principle of the doctrine is, however, universally received. I have, nevertheless, little doubt that every practical physician will agree with me, that the presence of uric acid or urate of soda cannot be the sole cause of this distressing malady. In Ireland, gout is one of the rarest affections met with in hospital practice, yet I have

repeatedly found the blood of otherwise healthy men lying in our accident-ward loaded with urates; and my own blood has been in this condition for years, although I have never had the least symptom of gout. Dr. Gairdner found urates in the blood of a boy four years of age, in whose family gout had never been known. It is therefore necessary to find another link to complete the chain; this, I think, is furnished by Cullen's theory.

The predisposing cause of gout is undoubtedly the presence in the blood of uric acid, and of soda in some form; nerve-force, I believe, when in a healthy condition, preserves these two in a fluid state, separately, in a condition in which they may be eliminated by the skin, kidneys, or bowels. As soon, however, as this nerve influence is lessened, these two substances unite in the tissues most removed from the brain and centre of circulation. Irritation and inflammation excite the nervous system to increased energy, and the disease for the time is arrested; often, however, a like cause produces a second exacerbation, a third, or even a fourth, and then nature gradually alters or removes that which has been deposited, and all traces of the fit have passed away. The time that an attack generally commences is at night, when both nerve-force and the circulation are weakest; the position, moreover, most usually affected—the great toe—favours my theory; and I need scarcely mention the numerous instances recorded where a fit of gout has been brought on by great nervous depression. Columbus never suffered from the disease until disappointment and ingratitude depressed his whole nervous system. Hundreds of similar cases are familiar to all; politicians and speculators are particularly liable to gout.

The action of colchicum furnishes me with one more proof. By an experiment which may be repeated by anyone at will, I have satisfied myself that it is a nervous stimulant. Repeatedly have I, whilst fasting and in perfect health, taken doses of from ten to fifteen minims of tincture of the seeds of colchicum. Its effect was to produce, first nausea, and then increased action of all the organs of the body; the skin became moist, the action of the kidneys and liver increased, and the mental faculties were invigorated. On some occasions the heart's action was much increased, and I have even experienced severe palpitation consequent on its use. All these results can only be produced by a nervous stimulant. This is the only way too, in which its apparently magical influence on a seizure of gout can be explained, and it accounts also for the injury which its too frequent use may produce.

In conclusion, I have to recommend for trial, in the treatment of chronic gout, some medicines from which I have, in my practice, derived the greatest assistance.



Sulphate of nickel and the triple phosphate of iron, quinine, and strychnia will be found of the greatest benefit.

There is but one other point connected with this subject which I desire to mention. It has often been doubted that the skin can eliminate uric acid; but if a large quantity of perspiration be collected, and, after evaporation, tested, abundant crystals will be obtained. For this experiment it is necessary, in the first instance, to have a large quantity; I have repeatedly obtained as much as a pint, by aid of the Turkish bath. From a small amount of perspiration I rarely succeed in obtaining positive proofs of its existence.—*Lancet*, July 27, 1872, p. 114.

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# 11.—ON THE TREATMENT OF RHEUMATIC GOUT BY THE AID OF THE CONSTANT GALVANIC CURRENT.

By Dr. JULIUS ALTHAUS, Physician to the Infirmary for Epilepsy and Paralysis.

[The author in this paper does not enter into the question as to the real nature of the disease. He remarks, in the introductory portion of the paper, that rheumatic gout is one of the few diseases which can be diagnosed by merely looking at the patient, without asking a single question.]

I will not here discuss the question whether rheumatic gout owes its origin to a special poison which is generated in the system. No proofs have been brought forward for the assertion that it is so, which has perhaps been somewhat too rashly and confidently made; pathologists relying for this view more upon the analogy of the disorder, with gout on the one, and rheumatism on the other, hand, than upon any very precise data at their disposal with regard to rheumatic gout itself. Taking all the special features of this distemper into consideration, it seems to me more rational to look upon it as a nutritive disturbance of the joints peculiar to old age, or premature old age. By far the largest number of cases of this disease are observed after the age of 45; and although instances do occur in which rheumatic gout occurs in much younger persons, yet we invariably find, when such is the case, that emotions, anxiety, privations, voluntary or involuntary offences against the laws of hygiene, and other debilitating influences, have produced a sort of premature *senile decay* in the persons thus affected. The morbid process which under these circumstances takes place in the joints, is indeed very analogous to the atheromatous process which occurs in the internal tunic of the arteries at the same period of life, and which nobody thinks of putting down to a special poison generated in the system. It constitutes one of



those highly *insidious and chronic inflammatory conditions which are characteristic of the period of involution and senile decay.*

This peculiar kind of inflammation, which, if it occur in the synovial membrance and the articular cartilages, we call rheumatic gout, or rheumatoid arthritis, or arthritis deformans, and which, if observed in the intima tunic of the arteries, we call endo-arteritis deformans, or the atheromatous process, never seems to lead to suppuration, as it is not intense enough for that. The first thing that occurs in the joints is hyperplastic proliferation of all the parts involving the same. The synovial membrane throws out, partly, somewhat large pediculated polypi, consisting of fat and connective tissue, and partly a finer kind of tufts and fringes, causing a peculiar roughness of the surface, which Volckmann has appropriately likened to a sheep's-skin. The cartilages likewise become proliferated, at first in their superficial and external layers, and subsequently in their deeper parts. In the healthy cartilage, *pairs of cells*, which look like the two halves of a ball cut through in its middle, are seen to be regularly imbedded in the hyaline basement substance; and the proliferation of these cells occurs in strict proportion to a simultaneous growth of that basement substance, so that always a proper distance is kept between the several cells. Now, recent microscopical researches have shown that in rheumatic gout, on the contrary, *groups of from ten to twenty large cartilaginous cells* are formed, which are lying close to one another, to the detriment of the basement substance, which becomes more fluid, absorbed, and finally altogether wasted. The natural consequence of this tissue-change is, that the joints are no longer able to stand the wear and tear, either of motion or of pressure, which is put upon them. The structure, therefore, gradually perishes, and proliferation continues only at the edges of the articular cartilages. There is, however, no uniform swelling there, but numberless small nodes are formed, which are as many ecchondroses, and which gradually coalesce to form a sort of wreath round the joint. The surface of the bone thus becomes gradually denuded. This, however, does not cause caries, but osteitis, terminating in sclerosis; and the bone becomes covered with a smooth, shining, white, and firm osseous plate, on which traces of the mechanical injuries to which it has been exposed during life may often be discovered. This brittle osseous tissue cannot, in the nature of things, resist in the long run the constant friction and grinding to which it is exposed, however quiet the patients may keep, in order to avoid the pain which is caused by every motion. If two pieces of pumice-stone were constantly rubbed against one another, only little would be left after a time; and in a similar manner the surface of the bone gradually wastes away. Whole epiphyses

may thus perish; the neck of the femur will disappear; and the affected joints will altogether be so deformed that only little, if anything, is left of the original structure.

Looking, then, upon rheumatic gout as a disease of malnutrition peculiar to the period of involution and senile decay, we must not be led away by the term "inflammation" into anti-phlogistic measures, which only make matters worse; but the great aim of our treatment should be to support and stimulate the failing powers. The remedies upon which chief reliance is placed by the best observers are, therefore, bark, mineral acids, strychnia, iron, cod-liver oil, and arsenic. There can be no doubt, from the positive statements of reliable practitioners, that in a considerable proportion of cases these remedies do a great deal of good: yet how frequently do we see patients who have for months and years been under the care of the most eminent physicians, familiar with all the ordinary resources of the art; who have had everything done for them which money can procure; who have not only taken gallons of physic, but paid one visit after another to Buxton, Bath, Wiesbaden, and Wildbad, and who yet find the distressing symptoms of their complaint gradually becoming intensified, the pain more severe, the deformity greater, the helplessness more embarrassing. Surely under such circumstances we cannot look with any degree of satisfaction upon our present treatment of rheumatic gout; and any means which promises, from general considerations, to be really effective in such cases, should have a fair trial given to it.

Holding these views, I have for the last six years sought and found many opportunities to use in this distressing distemper that most active stimulant of nutrition the continuous galvanic current; and, although I am far from considering it a specific or a panacea for all cases of rheumatic gout, yet I have seen enough of its effects to convince me that in cases where the most approved lines of medicinal and hydrotherapeutic treatment, intelligently and perseveringly followed, have, after months and years of trial, left the patient no better but rather worse than before, the constant current, judiciously administered, may do great good in a variety of ways. I do not mean to say that we can by its aid really cure rheumatic gout. The current cannot reproduce wasted cartilages, nor restore its normal structure to a sclerosed bone; it cannot prevent the patients from advancing in age, nor arrest altogether the process of involution and senile decay to which the system must in the end succumb; yet it can do much good. Instead of relating cases which might prove monotonous, I will shortly summarise the results of my experience about the therapeutical capabilities of the current in the disease now under consideration.



1. It acts as a general tonic to the system, and more especially to the nervous system. Although in some cases of rheumatic gout the appetite is tolerably good, the food taken is pretty well digested, and the action of the liver, the alvine evacuations, and the urinary secretions, are not very materially interfered with; yet we find that the large majority of patients suffer from some form of dyspepsia, and impaired action of the liver, bowels, and kidneys. In the cases to which I now allude, there is no actual disease of these organs, which sometimes becomes developed towards the end; but there is functional debility, owing to an insufficient supply of nervous force to the parts mentioned. Thus an insufficient quantity of food is taken; that which is taken is imperfectly digested; the elimination of effete matters from the system is slackened; and the consequence of all this is a degree of debility which is more alarming to the experienced observer than the discomfort and pain attending the local manifestations of the disease in the joints. Patients of this class have often considerable difficulty in taking medicinal tonics, which are apt to heat the system, while alteratives often lower it still more. Under such circumstances the constant current is often remarkably effective in rousing nervous energy and improving all the most important functions of the body. The best mode of application for this purpose is to direct the positive pole, armed with a conductor of large surface, to the cervical spine, while the negative pole, similarly armed, is placed on the pit of the stomach. A gentle current, which even patients of exquisite sensibility can well bear, is thus allowed to flow for from three to five minutes in the direction just described. By this application the base of the brain, the spinal cord, and the coeliac plexus of nerves, are simultaneously brought under the influence of the current.

2. *Want of sleep* is one of the chief complaints of patients suffering from rheumatic gout. This arises partly from want of exercise in the open air, and partly from the circumstance that the pain is apt to get worse at night. The patient keeps tossing about in bed from being unable to find a really comfortable position. "Nature's sweet restorer" thus flees the pillow of the sufferer, who generally dreads the night as his worst time. Opium and morphia are rarely applicable in these cases, as they generally excite rather than soothe, and impair the action of the stomach and the liver still more. Hydrate of chloral often steps in as the right thing in the right place; but I believe you will agree with me when I say that the more ample our experience of that remedy becomes, the more frequently do we find cases in which the system does not tolerate it, either in small or large doses. Where morphia and chloral fail to procure sleep, few other drugs will succeed in doing so;



and it must, therefore, be looked upon as a considerable advantage connected with the use of the constant current, that under its influence sleep soon becomes prolonged and refreshing. No special application is necessary for attaining this end, the one I have just mentioned being quite sufficient.

3. *Pain* is another invariable symptom of rheumatic gout, and one which frequently resists the administration of sedatives and counter-irritants. Even subcutaneous injections of morphia and atropia have only a temporary effect, which rarely extends beyond twelve or twenty-four hours. The pain is generally most severe in the deformed joints, but is likewise felt in many other parts of the body. For the alleviation of the pain, the constant current is the most useful remedy with which I am acquainted. It must in this instance be applied to the suffering parts, so that the positive pole, armed with a small electrode, is made to touch the sore points; while the negative, connected with a large electrode, is placed in the neighbourhood. The current used may in this instance be somewhat more powerful than where it is employed for general tonic effects; and if a short application, say of one or two minutes, be not successful, this may be extended to four or five minutes. The effect is, in many instances, almost magical, inasmuch as pain, which has been fixed in some particular spot for months and years, is, as it were, charmed away by one or two applications.

4. The *deformities* which rheumatic gout produces, more especially in the interphalangeal joints of the hands and feet, resist the use of the current more obstinately than the other symptoms which have been mentioned; and it requires much patience and perseverance on the part of the practitioner and the patient to reduce them to any extent. Provided, however, that the galvanic treatment is followed up for a sufficient length of time, even extensive deformities may be improved, which is a matter of considerable importance, as most patients belong to the female sex. No one who has not seen it can have any idea of the beneficial effects which the current will produce in the long run on the deformities, even where they are extensive, and where the patients are not very old. It seems as if the action of the vasomotor nerves were gradually directed back into its proper channel. Unfortunately, many patients want to be cured in a week of a disease which is essentially chronic in its progress; and some practitioners are inclined to discontinue a special treatment too soon, unless immediate effects are produced by it. In this as in many other complaints the ultimate success of a particular plan of therapeutics depends not alone on the remedies which are used, but also very materially upon the more or less systematic use made by the practitioner of his remedies; and last, not least, upon the intelligent co-operation

of the patient. In order to reduce deformities, galvanisation of the cervical sympathetic nerve must be had recourse to.

By what I have said I do not by any means wish to imply that the use of internal remedies should be altogether eschewed in the treatment of rheumatic gout. If we were, for some reason or another, to abstain completely from their use, we should only deprive ourselves of many chances to do good to our patients. Although in the system of therapeutics which I have pursued for rheumatic gout, I have given the first place to the constant current, I have frequently prescribed medicines which I thought would prove beneficial for removing certain complications which were present. Thus where there was an excess of acidity in the system, as evidenced by a highly acid condition of the mucous membrane of the tongue, the perspiration, and the urine, I have given alkalies or bismuth; for constipation, I have ordered Marienbad or Frederickshall mineral water; where the blood appeared to be very much impoverished, the ferrum redactum of the Pharmacopœia, or Spa water, was prescribed. All these and some other remedies I have used were, however, only auxiliaries of the galvanic treatment, and could not of themselves have exerted any decided influence upon the progress of the disease.

In conclusion, let me impress upon you once more the necessity of persevering with the use of the galvanic current, if it is to be used at all. The circumstance that most of our patients will be above forty-five, and not a few above sixty years of age, in connection with the obstinate character of the malady itself, must prepare us for slow results, more especially as far as the improvement of the deformities is concerned. Pain, indeed, is often relieved at once, but it is very apt to return; the patient often becomes rapidly stronger, but at times the old debility will again be felt, more especially if he undertake to do more, either in business or pleasure, than he is able to do; and a sleepless night may again occur after many good nights in succession. It is therefore necessary to tell the patient at once that a rapid cure is quite out of the question. The current should be used, either daily or three or four times a week, for at least a month or six weeks; and if the patient continue to improve, we may go on with it much longer than that. If, however, after that time he does not seem to make any further decided progress, it is better to discontinue the treatment for a month or longer, and then to recommence it. Those who will not shirk the sometimes rather tedious labour involved in carrying out such treatment perseveringly, will ultimately be amply rewarded for their trouble by an amount of success which, in some forms of the disease, could not have been obtained by any other line of treatment.—*British Med. Journal*, Sept. 28, 1872, p. 348.



12.—ON A CASE OF ACUTE RHEUMATISM, WITH HEAD-SYMPTOMS AND HIGH TEMPERATURE, SUCCESSFULLY TREATED WITH BATHS.

By Dr. HENRY THOMPSON, Physician to the Middlesex Hospital.

[It was formerly supposed when head-symptoms supervened in a case of acute rheumatism that it was meningitis or metastasis. It is now thought to be hyperpyrexia.]

Beyond question, inordinate body-heat is a destructive agent of great power, and capable of doing a vast amount of mischief to the nervous and other systems; nevertheless, in these particular cases any one who examines the records scattered through the journals and elsewhere, will at once see that the law is this: the nerve-symptoms invariably precede, and the hyperpyrexia, however it may intensify, or seem to intensify, those symptoms, invariably follows. The law may be annulled hereafter; but as far as existing data go, it must stand. The highest recorded temperature on the first accession of the nerve-symptoms in the cases now under consideration appears to have been at most  $105.5^{\circ}$ . In a large proportion of cases the temperature is below this level, and oftentimes considerably below it. Clearly, then, on the assurance of all past experience, the mischief originates in some profound and damaging impression upon the nervous system—an impression which gives rise at once to the phenomena of cerebral or cerebro-spinal derangement, and to those of exalted body-heat. They are in the main coincident effects of one common cause. I have dwelt upon a point well known to those who, personally or by reading, are familiar with the cases in question, but much misapprehended by many intelligent and well-informed persons. Be it so, you will say; but at any rate it is the hyperpyrexia which plays the principal part in the destruction of life. I am not so sure of that. Any one who watched the progress of my own case for many days before the invasion of anything like excessive fever-heat, and took note of the delirium, the tremors, the subsultus, and the floccitation, must have felt assured that the boy's life was in jeopardy, whether the temperature should rise to the height of hyperpyrexia or not; as much in jeopardy as that of any one who presented the same symptoms in enteric fever or in variola. Yes; but the bath, and the wet sheet, and the ice-bag, reduce temperature, and at the same time restore consciousness, or it may be, save life itself. It follows, therefore, you will say, that, as so much good ensues upon the removal of the hyperpyrexia, so, when the result is unfavourable, the hyperpyrexia is mainly responsible for the evil. The reasoning is inconclusive. There may be many factors at work, all con-



spiring together to compass the death of the patient. If the aggregate of these factors be overwhelmingly strong, there is an end of the matter, and the patient dies; but let the powers of life and death be more evenly adjusted, then the removal of any single factor (say the temperature) turns the scale in favour of life; or, to use a more homely metaphor, takes off the last ounce that is breaking the camel's back. You cannot, then, conclude with certainty, that in fatal cases the hyperpyrexia has done all, or nearly all, the work of destruction, even if you regard the processes of refrigeration as resulting simply in the reduction of heat: still less are we at liberty to draw this conclusion if we change the point of view altogether, and presume (as not unreasonably we may) that our refrigerating agents strike home at the root of the evil, and directly antagonise the first deleterious impression on the nervous system by producing a salutary counter-impression, which controls alike the hyperpyrexia and all the associated elements of death. The evolution of heat may be paramount from beginning to end in sunstroke; but in sunstroke the rule is, that a temperature averaging  $107^{\circ}$  precedes, and the nervous phenomena follow. In rheumatism, it appears to be the reverse. Nevertheless, it is universally believed that a continuous body-heat, ranging from  $108.6^{\circ}$  to  $112^{\circ}$ , is absolutely incompatible with life; and I dare not gainsay that belief. Now, in the terrible cases of which I am speaking, the temperature, if unchecked, runs rapidly up to the aforesaid range, and, if unsubdued, must inevitably issue in death. It behoves us, therefore, practically to accept the dogma, that it is the heat which is the main destructive element, and to act upon it at the bedside, whether we believe it or not. We can control temperature, and we are bound to control it. This, in my judgment, is best accomplished agreeably to the plan originated in this country by Dr. Wilson Fox, by the use of the bath, at a temperature of  $90^{\circ}$  or  $95^{\circ}$  in the first instance, gradually reduced by the addition of cold water to  $70^{\circ}$ , or even to a lower point in some cases. I confess I have no personal experience of the wet pack and the ice-bag; but many of those who assisted me had such experience, and they all emphatically declared their preference for the bath, as at once the easiest, the most manageable, and the least distressing of all the appliances. In our case, at any rate, the bath was all-sufficient for the purpose required; but it is possible that our case was exceptionally amenable to treatment.

And now, you will naturally ask me whether we are to be perpetually wielding the thermometer, registering temperatures, and constructing charts in every case of acute rheumatism. If we are sure of the ground we have taken, on a foundation of some five years' standing, such a proceeding would be a

lamentable loss of time. Recollect what I said about the invariable antecedence of the nervous phenomena in the small minority of cases accompanied by hyperpyrexia. If, therefore, the law stand fast hereafter, as it has stood hitherto, you may in the main rest contented with the ordinary routine of daily registration two or three times in the twenty-four hours, and wait until one or more of the nerve-symptoms arouse the suspicion of impending peril; but you must give a large latitude of meaning to the term "nerve-symptom," and you must be keenly alive to everything that can fairly be included under it. I will enumerate all the prodromata of nervous origin that I have been able to gather from my own experience and from the cases recorded within the last five years—at least from those recorded in detail, and bearing conclusively upon the point in question. They are delirium, restlessness, rebellious to treatment, and, it may be, disproportionate to pain; subsultus, tremors, disorderly respiration, stupor, somnolence, deafness, prostration, apathy, strangeness and waywardness of manner, unnatural anxiety and depression of mind; even unnatural gaiety and garrulity. Let any of these sound the note of alarm, and the thermometer, how slight soever its value by comparison hitherto, becomes invaluable now. It is now your bounden duty to apply it many times in the day: how many times, it is not easy to say. I will give you roughly my own ideas on the subject as far as they go. If there be delirium alone, or delirium with extreme restlessness, I should recommend the temperature to be taken at least every two hours. I gave these instructions in my own case. It is true the boy was naturally excitable in a high degree, and the case was an unpromising one from the first; but, under any circumstances, I should look upon a longer interval as unsafe, with a certain reservation for the contingency of sleep. After all, your labour will often be lost. Delirium and restlessness, as a rule, pass away, and leave nothing behind; nevertheless, they are now and then true prophets of evil, and their warnings are never to be disregarded. In cases of this nature I should not allow the temperature to go beyond  $106^{\circ}$  before administering the bath.

As for the remaining prodromata on the list, it is difficult to estimate with precision their respective values: there are two, however, which I should regard with especial misgiving—over-anxiety and stupor. In the presence of stupor I should take the temperature every hour or every half-hour; indeed, I should look upon the arrival of a climax as all but a certainty, and I should plunge the patient into a bath the moment the thermometer marked  $105^{\circ}$  or  $105.5^{\circ}$ . The case is somewhat different with over-anxiety: we might easily alarm the patient by perpetual interference; and it would be obviously inexpedient to



disturb a calm and natural sleep, which might fairly be assumed to betoken that all was going on well. Under all circumstances, it is imperative to have the room cleared for action, and the bath and all its appliances in readiness. It is unsafe to be caught unprepared on this first ominous rise in the thermometer, for the interval between the event and the acme of the hyperpyrexia is sometimes fearfully short.

If, in reference to the possibility of overlooking the nervous phenomena, or in order to make assurance doubly sure, you require collateral evidences of approaching danger, I will mention a few symptoms which are held to be of peculiar significance. A dry unperspiring skin; a profuse and intensely red miliary rash; an abatement of the pain and swelling in the joints, or their total disappearance; diarrhoea, and a copious flow of pale alkaline or neutral urine, have all been observed to accompany or to precede the paroxysms; they may even precede the nervous prodromata, specially so called. In our case, a free flow of urine occurred before delirium, and diarrhoea occurred with it. The articular pains, for many days before the outbreak of delirium, appeared in all the joints, and disappeared from all in the most desultory way. Again, during the four days prior to that outbreak, there was a slow rise in the temperature, which on the fourth evening culminated at  $104.2^{\circ}$ ; the pulse-rate, also, during the same period, ruled higher than before; but the complexity and continued severity of the case seemed sufficient to explain these occurrences. An unexplained rise in either direction would certainly be a suspicious symptom: perhaps even a rise of any kind should put us on our guard. Unfortunately, the collateral evidences are all inconstant, but they are useful auxiliaries, and when they appear they ought to awaken attention. Let me remark, by way of parenthesis, that, as I should be vigilant myself, so I should inculcate the extremest vigilance on every one entrusted with the care of the patient; and I should direct the attendants on the appearance of anything strange in his aspect or demeanour, to use the thermometer assiduously, and if ever it rose to  $105^{\circ}$ , to send at once for the physician.

I have laid down the law that, for all practical purposes, as far as the bath is concerned, we must take the thermometer for our guide. I wish, in some measure, to modify this hard and fast rule. So singularly calming is the bath, even when there is no great exaltation of the body-heat, that I should be disposed to administer it as a simple palliative in all exacerbations of delirium and restlessness, although the heat may have fallen far below its point of maximum intensity, and may register at the time no more than from  $102.5^{\circ}$  to  $104^{\circ}$  in the axilla. At a lower level than the above it would not be desirable to use the



bath, for such temperature would not allow a sufficient range to cover both the fall within the bath, which we can regulate, and the fall after the bath, which we cannot regulate with exactness; although, from a comparison of Dr. Wilson Fox's cases, I should roughly reckon the average for the after-fall at about four degrees. On the foregoing plan our last bath was administered, when the temperature was no higher than  $103.1^{\circ}$  in the axilla, and it accomplished its purpose. Perhaps I might have adopted the same plan with advantage during the prolonged agitation which preceded the climax. In this connexion I would remark that, with a temperature descending gradually from  $95^{\circ}$  or  $90^{\circ}$  to  $70^{\circ}$  or thereabouts, the bath is at least as much a warm bath as a cold bath; and warm baths have for ages been recognised as sedatives to the nervous system.

Before dismissing the subject of the bath, it is important to observe that the chest-complications, severe and extensive as they were, including pneumonia, pleurisy, bronchitis, and pericarditis, underwent no perceptible change for the worse in consequence of its use.

With regard to medicinal remedies, opium and hydrate of chloral are the only soporifics and sedatives of any real value. They may be given freely enough during the premonitory period, or stage of suspicion, to control mere delirium or restlessness; but if I saw the danger-signal in the thermometer, I should put on the break at once and stop the opium for the time being. I might resort to it again if, after the subsidence of the hyperpyrexia, the agitation persisted, and chloral failed to soothe it. Under these circumstances, we cannot afford to dispense with opium altogether; but let it never be forgotten that there is no sharp and unvarying line of distinction between the comatose and the delirious class of symptoms; that stupor may alternate with excitement, and that death by coma is the natural termination of the attack.

In cases characterised by stupor from the first, with or without alternations of excitement, you may apply ice to the forehead or scalp, or the so-called revulsives to the trunk and extremities. I confess I should be afraid of blisters, wherever applied. As the symptoms progressed, I should trust principally to the early employment of the bath, as before stated. In all cases, of whatever type, I should feed and stimulate the patient largely, and I should take especial care not to weaken him in any way.

You are aware that modern researches have established for quinine a high reputation as a refrigerant; but in all cases that I have perused, I fail to find any illustration of this reputed power. In my own case, as the boy happened to be doing well,

on the principle of letting well alone I was unwilling to alter one iota of the treatment, and so he remained for days and weeks under the influence of quinine, whatever that might be. I cannot determine its value; and the same thing is true of the digitalis associated with it. The possible production of cinchonism is a great drawback to the use of quinine, and might embarrass the prognosis in a class of cases where deafness may be a spontaneous symptom, and a dangerous one.—*British Med. Journal*, August 3, 1872, p. 113.

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#### DISEASES OF THE NERVOUS SYSTEM.

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### 13.—SCIATICA, LUMBAGO, AND BRACHIALGIA; THEIR NATURE AND TREATMENT, AND THEIR IMMEDIATE RELIEF AND RAPID CURE BY HYPODERMIC INJECTION OF MORPHIA.

By Dr. HENRY LAWSON, Assistant Physician to St. Mary's Hospital.

[The following is from a review of a work on this subject recently published by Dr. Lawson.]

Dr. Lawson is aware that in recommending his plan, he exposes himself to the charge of treating a symptom only; but, in the case of sciatica, he believes that "the symptom (pain) is the whole disease; and by arresting it we remove the affection." He is of opinion that at present we know nothing of the pathology of sciatica, and that its connexion with that vague class of diseases, neuralgia, is questionable. He is also strongly opposed to the theory that sciatica is an affection of the central nervous system, as its classification among the neuralgiæ by some authors would seem to indicate, but suspects that the portions of the nerve structure primarily attacked, are the delicate filamentous reticulations on the surface of the sarcolemma.

Upon the subject of the treatment of sciatica, the remarks of Dr. Lawson are entitled to weight, as he speaks from personal experience in his own case, of the value of nearly every remedy that has ever been lauded as a cure for the disease; and the conclusion he comes to is, that "the true and *almost the only remedy for sciatica*, is hypodermic injection of minute quantities of morphia." The solution our author recommends, differs somewhat from those in ordinary use; it is simply composed of ten grains of hydrochlorate of morphia, dissolved in two drachms of distilled water, without any acid or glycerine. This preparation is solid at ordinary winter temperatures, and



generally so in summer, and, consequently, must be heated before each injection; it possesses, however, the advantage that it escapes decomposition, which so frequently occurs in other solutions. One-sixth of a grain of morphia. *i.e.*, two minims of this solution, is the proper commencing dose. The effects produced by the operation—which it is well to perform immediately after a meal, and in the locality of the pain—are: “absence of pain, intense comfort for a time, prolonged sleep, and increased appetite.” These results seldom last longer than twenty-four hours, and it will be well then to repeat the dose, and, if requisite, increase its strength. In severe cases, it will be necessary to inject twice a day, and the hypodermic use of morphia may have to be continued for many months. The prevention of the pain of the injection—with some timid patients an important point—may, as the author reminds us, be secured by producing a moderate degree of anæsthesia of the skin by means of the ether-spray apparatus.

Dr. Lawson does not seem to have observed any bad results from the employment of morphia in this form, beyond a sensation of nausea experienced by some after the operation, which, he states, may be relieved by taking food. We regret, however, that he has given no caution against the danger which the prolonged use—or rather abuse—of the hypodermic injection of morphia, may sometimes produce. Although temporary immunity from suffering is always procurable by a sufficiently large dose, yet, in some protracted cases, there is, when the influence of the morphia has passed off, a return of the pain, and a craving for a repetition of the injection, which engenders a “morphia-habit,” difficult to abandon, and which, from a reliance upon the hypodermic treatment only, may possibly, as suggested by Dr. Clifford Allbutt, “perpetuate the morbid condition under the mask of a seeming relief.” It must not be supposed, however, that although Dr. Lawson believes that 90 per cent. of the cases of sciatica can be cured by the hypodermic injection of morphia alone, he ignores altogether the assistance to be derived from other sources. Cod-liver oil, iron, and quinine, he has found very valuable, in “that they help to maintain the nutrition of the body, which the agonizing pain of the malady so greatly interrupts.” The employment of alcohol—whether as brandy or whisky—in severe sciatica, is, the author says, “to be uniformly insisted on;” but beer, wines, tea, and coffee, are to be avoided.

In the treatment of lumbago and brachialgia, Dr. Lawson has found the hypodermic injection of morphia as useful as in sciatica.

Herpes zoster, which so frequently accompanies lumbago, and intensifies the sufferings of the patient, is also relieved by



the hypodermic injection of morphia, and by painting over the vesicles, immediately they appear, with styptic colloid.—*Dublin Journal of Med. Science, June, 1872, p. 478.*

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14.—CASE OF TRAUMATIC TETANUS, SUCCESSFULLY TREATED WITH CHLORAL AND BROMIDE OF POTASSIUM.

By Dr. ALEXANDER FERGUSON, Peebles.

Tetanus is in itself such a terrible disease, and the issue, especially in the traumatic variety, so commonly fatal, that one aggravated case successfully treated is worth recording. I therefore make no apology for submitting the following interesting case:—

M. D., aged 20, a sawyer, was brought to my surgery on the morning of 19th April last, having sustained an accident to his hand while engaged with a circular saw. The injury was of such a nature as to necessitate my amputating the thumb close to the hand. The operation was done under chloroform, and dressed antiseptically with the utmost care. Under this treatment it cicatrized kindly until the morning of the tenth day, when I was urgently called to his house. I found to my horror the jaws firmly clenched, the characteristic “risus sardonicus” distinctly marked, and the body slightly opisthotonic. The wound was inflamed and painful, and the healthy secretion dried up. A large linseed poultice, slightly carbolized, was applied, and 30 grs. of chloral hydrate given, its sedative effect being promoted by a whiff of chloroform. In the evening, symptoms aggravated, the opisthotonos being more complete. Repeated the chloral, increasing the dose by 10 grs. Next morning patient is decidedly better, complains chiefly of præcordial pain extending through to the back, and probably diaphragmatic. Continued the chloral treatment as follows: 40 grs. ter in die, alternately with 30 grs. bromide of potassium bis in die, to every dose of which are added 10 grs. of chloral. Under this plan of treatment, the effects of which resembled chronic alcoholism, the spasms were kept at bay. The only additional treatment was the hot bath with mustard for the first four nights. In the meantime, my friend Dr. Joseph Bell of Edinburgh was asked in consultation, and was so satisfied with the treatment as to recommend its continuance. If the symptoms got aggravated, he advised Calabar bean, of which he kindly left a supply. There is one incident of pathological significance that occurred in the management of this case. One morning, when dressing the wound, I passed my finger along the cicatrizing surface to repress exuberant granulations. It came in contact with a sharp point. This I found on examina-

tion to be a small sequestrum of comminuted bone, the removal of which was followed by rapid cicatrization. Such was the treatment exclusively adopted as far as medicine is concerned. The dietary consisted of farinaceous articles, with beef-tea and milk, and small quantities of whisky and soda-water.

*Remarks.*—There are one or two points of surgical interest in connection with the above case.

The treatment is, I conjecture, novel as regards the use of the bromide of potassium. I was induced to try this remedy for two reasons. In the first place, it is harmless, even in very large doses long continued; and secondly, its therapeutic effects are well known in epilepiform diseases, to which, in the absence of more pathological enlightenment, tetanus seems to me closely allied. At all events, the abatement of muscular spasm, best observed in the trismus, shows that, though pathologically I may be wrong, therapeutically I am right.

The next point of pathological interest refers to the little spiculum of bone, extracted from the cicatrix. Its removal was followed by a very marked remission of the symptoms. I write under correction, but an analagous case I think happened in the practice of Dr. Ebenezer Watson of Glasgow. The removal of a small gravel stone from a lacerated wound of the hand in a tetanic patient was followed by a corresponding abatement. This is surely a point of surgical importance, not to be overlooked in such cases. Here was detected and removed a direct cause of reflex irritation.

My case also exemplifies how chloral may be borne in suitable cases. For four weeks he has taken 120 grs. daily of chloral, and 90 grs. bromide of potassium (in the aggregate 3600 grs. of the former, and 2700 grs. of the latter), without any appreciable effect on the general health beyond transient loss of memory, and the ordinary manifestations attending intoxication. He is now able to walk through his room, but until every sign of spasm has disappeared he will continue to take the chloral.—*Edinburgh Med. Journal*, July, 1872, p. 37.

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#### 15.—ON A CASE OF TRAUMATIC TETANUS.

By DR. G. W. ROBERTS, St. Asaph, Flintshire.

Hugh J——, aged fifty, farmer, a dark, short, thick-set man, on Feb. 16th received a wound on the back of his left hand which was of an extremely lacerated nature. For the first fortnight after the injury he did well until he attended a sale of live stock and got cold. The same evening he complained of great pain in his hand, and also of having a stiff neck. Next morning I was summoned to see him, and found him suffering



from great pain in the hand and a contracting pain in the pit of the stomach through to the back; his voice was weak and feeble, and the muscles of the neck and face contracted; pulse 100. I ordered a poultice for the hand, and prescribed a simple saline mixture with ten grains of Dover's powder to be taken immediately. In the evening I was sent for again, and found him attacked with spasms of the trunk and lower extremities. The abdominal muscles were hard and contracted. Spasms were produced by the most trivial cause, such as a draught of cold air by opening the bedroom door. Even the slightest noise made in the room would bring on the most intense tonic spasms of the trunk and lower extremities. These spasms produced complete opisthotonos of about generally two and a half minutes' duration. The features wore a peculiar expression, as if the patient was constantly laughing; the angles of the mouth were drawn downwards and sideways, the forehead wrinkled, brows knit, eyes sunken and fixed, pupils dilated; the pulse was weak, and rose as high as 140; the body was bedewed with a cold clammy perspiration; the temperature rose as high as 107° Fahr.; had great difficulty in swallowing liquids.

My treatment from the first outbreak of spasms was by the subcutaneous injection of a forty-eighth of a grain of solution of atropia twice a day, half-ounce doses of succus every three hours, and half a grain of muriate of morphia at bedtime with the object of inducing sleep. I held a consultation with my colleague, Dr. E. Pierce of Denbigh, who, in addition to the above treatment, advised thirty minims of tincture of Indian hemp every three hours, also a poultice of tobacco and opium for the wounded hand. Strong beef-tea and brandy was administered, which the patient managed to take with very great difficulty. We had also recourse to injection of beef-tea, port-wine, eggs, &c.

On the twelfth day following the first spasm my patient was free from any spasmodic attack, and remained so, but the stiffness in the neck and extremities continued for some considerable time. By the end of April he was perfectly recovered.—*Lancet*, Sept. 7, 1872, p. 333.

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## 16.—CHLORAL IN DELIRIUM TREMENS.

By Dr. JOHN BARCLAY, Banff.

I was called late one night to see a young man who was labouring under this complaint. He had attempted to hang himself, had been rescued, and was then very violent. About half an hour before I saw him he had got half a drachm of solution of morphia, which seemed to have no quieting effect whatever. I then injected hypodermically half a grain of

morphia, and waited an hour; but at the end of that time he was still as violent as ever. I then injected half a grain more, and waited for two hours; but even then the violence was, I think, if anything, greater than before. I now sent for a drachm of chloral in solution, half of which I gave him. In ten minutes he was fast asleep, and he slept for ten hours. When he awoke he was slightly collected; he was well fed with strong soup and arrowroot, got the rest of his chloral draught, fell asleep again, and in twelve hours more awoke almost rational.—*Lancet*, Sep. 21, 1872, p. 405.

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17.—A CASE OF PARAPLEGIA LASTING FIVE YEARS;  
ELECTRICAL ANÆSTHESIA.—QUESTION OF  
MALINGERING.

By Dr. W. MOXON, Assistant-Physician to Guy's Hospital.

A lad aged eighteen was admitted into Guy's Hospital (Astley Cooper ward), under my care. He had been sent up from Northampton with the following history: He had fallen from a hay-loft nearly six years before admission, and in consequence of the fall was laid up with weakness of his lower extremities and of his back; he said that his water had been drawn off on account of his inability to pass it. The history which the lad gave was tolerably consistent. He said he had fallen from a height upon his back, and that paralysis of the bladder and lower extremities was the result of the fall, all of which was very likely.

Whenever a paralytic case comes under care one gets an advantage by inducing the person to put out all the effort he possibly can, urging and compelling him to do so. In this way one learns much of the nature of the ease by the degree of willingness with which effort is put out, the freedom of the effort in the parts actually moved, as well as, of course, by the kind and degree of the imperfection in the usefulness of the paralysed part.

On making the boy get out of bed, he flopped on the floor, propping himself on his arms and resting on the side of his buttock, with his legs flexed at all the joints so as to be drawn up rather tightly. The legs were remarkably blue and cold, especially below the knees. The suspicious points about the lad's case were—first, the total paralysis he claimed to have; and, second, the defined line around his knees at and below which he declared the sensation to be lost. Under chloroform it was found that his legs, which he said had hurt him to straighten, would stretch to their length quite naturally; and when cautioned to be very careful, at the same time that he



was forced to get upright—being held up in the air, in fact, by the arms,—his legs made very promising movements.

Testing the exact line limiting his anæsthesia in the way we employed led to still more doubt. We marked the professed line with ink, then made him shut his eyes, and tried his sensibility to touch above and below it. With his eyes shut he could not keep to his line as he had done most accurately when he was looking at it. It was remarkable, though, how near he kept to it.

The most interesting results were obtained from the electrical excitation of the paralysed parts. The lad was a miserable coward, and got into insufferable terror at the approach of the electrical battery. We did not calm his fears, but gave substance to them by applying the interrupted current so strong that I could scarcely bear it myself, which I endeavoured to do for the sake of appearances. He fairly bellowed when the poles were applied to his arm, the stimulated muscles of which contracted very actively, but I was much surprised to find that when this strong current was applied to his legs below the knees, he gave not the slightest sign of feeling it, and said that he scarce felt it at all, while the muscles contracted so feebly that their reaction was often doubtful, and never more than very trifling. The same current applied in the same way to the legs of a man in the next bed, a courageous fellow who willingly underwent the experiment, made him hiss inwards through his teeth, and no effort of will on his part could prevent the muscles from strongly contracting, though he made a very energetic face in the endeavour to resist contraction, whereas the boy's countenance was perfectly placid while the electricity was applied to his legs. The boy had been a fortnight in the hospital before I made this examination by electricity, and had already made considerable progress; first he had stood up holding on to the back of a chair, then pushed the chair before him, and at last lifted the heavy chair, supporting it instead of having it to support him. As all this mending had occurred in a fortnight, and especially as I could not but think that the power required to lift a heavy chair, in the awkward stooping posture the boy always assumed, was far greater than compatible with his claim to be paralysed, I had come to regard him as a shammer. These results of electrical examination, however, made me at the time doubt whether it could be all sham, and led me to expect that perhaps a check in the progress of the case might be anticipated. I therefore adopted a more active treatment than that hitherto used, which had consisted of camphor-water and driving behind the chair. The fresh means consisted in the use of silver, administered, not internally, but in the form of sixpences, and accompanied with

promises of more. Under all these therapeutical means his progress was wonderful. He left go the chair back and got about on a broom-handle, then learnt to carry the broom. Five weeks after Christmas he ran a very good race with another boy in the hospital-park and won the shilling. He is now apprenticed to a tailor in the city of London, and described by his friends as doing well.

This issue of the case I suppose will be taken to show the boy's condition throughout was no other than deliberate deception. I can't help holding that opinion. Under these circumstances the state described in regard of electricity becomes of great interest.

Can we suppose that the prolonged disuse of the limbs had so brought down the sensitiveness of his legs and weakened their muscles that the usual painful contraction was partly not set up and partly not felt. It is difficult to suppose this, seeing that the power of the will found its ways into his legs so soon and thoroughly as the story shows. Nevertheless, I cannot suggest any more likely explanation of the difficulty, and we must remember that although recovery was quick it was gradual. I should, however, have said that in the latter few weeks the electrical current was used to the boy's legs; but the recovery was by that time complete. I ordered the electricity only for its moral effect; yet some may be disposed to think that that helped the cure.

But as my belief is that the lad was malingering, I think the chief interest of his case lies in its showing that want of absence of reaction to the electrical current does not prove a paralysis to be real; while the whole history shows the power over such cases which one may exercise by firm and persevering pressure upon the patient's will, keeping up a sufficient friendliness in the meantime.—*Lancet*, July 6, 1872, p. 7.

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#### 18.—GUARANA A REMEDY FOR SICK-HEADACHE.

By DR. SAMUEL WILKS, Physician to Guy's Hospital.

I wish to draw the attention of the profession to *guarana* as a remedy for sick-headache, and at the same time to ask for the experience of those who may already have some acquaintance with the drug. My own knowledge of it dates about two years back, when, after the appearance of a lecture of mine upon sick-headache, I received a letter from Mr. Helmcken, of British Columbia, enclosing two powders which he recommended to me with much confidence as able to cure the complaint. He said that, having heard much of the remedy, "I resolved to try the medicine upon one of my patients who was always coming to me with sick-headache; and sure enough it



acted like a charm ; and in place of suffering for twenty hours or so, the headache had disappeared in a couple. This accords with what others have told me." Upon my first headache after the receipt of Mr. Helmcken's letter, I took the powder, but with only doubtful effect. I therefore did no more than casually mention the medicine to my friends, but did not recommend it. A few weeks ago, after the appearance of a second communication of mine in the Journal upon the same complaint, I received a letter from Dr. Wood, of Montreal, in which he also recommended "guarana" as a remedy for headache, and gave a history of his own personal sufferings and the relief which he had obtained. He says "By taking one of these powders and remaining quiet when I have felt premonitory symptoms by a beginning of pain always in the right temple (headache on the other side, or in any other part of the head, I never mind), I have carried off the attack ; and, with the first box, absolutely put it off for two months—something which had never occurred in my life before." Upon so good an authority, I determined to try the remedy in a more systematic manner, and requested my neighbour, Mr. Hooper, the chemist, to procure me a packet of the powders. These I have recommended to several patients and friends ; and the result is so encouraging, that I have hastened to suggest their trial to my professional brethren. One lady speaks most enthusiastically of their power, as she has now, on two separate occasions, had her headache arrested by their use. The drug has long been known, for mention is made of it in English and French pharmacologies, but appears never to have come into general use. It consists of the seeds of a tree growing in Brazil called *Paullinia sorbilis* ; and these, according to Johnstone, in his *Chemistry of Common Life*, are used as we do cocoa. The seeds are ground into powder, and contain an alkaloid which is said to be identical with that found in tea and coffee. The medicine is manufactured by Grimault and Co., No. 7, Rue de la Feuillade, Paris.—*British Medical Journal*, April 20, 1872, p. 421.

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#### 19.—GUARANA POWDER A REMEDY FOR SICK-HEADACHE.

By Dr. P. W. LATHAM, Physician to Addenbrooke's Hospital, Cambridge.

The above mentioned remedy, to which Dr. Wilks called attention in last week's journal, has for some time been a favourite one in France for sick headache, as well as for various forms of neuralgia, given in doses of ten to fifteen grains or more of the powder once or oftener in the day. It is now also, according to Dr. Herman Lebert, coming into use in Germany

(*Handbuch der practischen Medicin*, 3te Aufl., 1863, Bd. ii, s. 632.)

The following account (abridged from Posner's *Handbuch der Klinischen Arzneimittellehre*, 1866, p. 189) may not prove uninteresting to those who are desirous, on the recommendation of Dr. Wilks, of giving the remedy a trial, and who may wish to know something of its properties.

The plant *Paullinia sorbilis* is a native of Brazil, and belongs to the natural order Sapindaceæ. The seeds, roasted, bruised, and pressed into cylindrical masses, constitute the guarana paste, which, when freely pounded, is then known by the name of Paullinia powder. This is light brown in colour, has an odour faintly resembling roasted coffee, and a bitter astringent taste. It contains, in addition to empyrenumatic oil (developed by the process of roasting) and tannic acid, guaranin, a substance identical in composition with caffein. The large proportion of tannic acid in its composition renders its action very efficacious as an astringent; whilst, owing to the guaranin, it has much the same effect on the nervous system as tea or coffee.

Therapeutically, Paullinia powder is employed, not only as stated above, to act on the nervous system in sick-headaches and various forms of neuralgia, but also as an astringent in catarrhal diarrhœa, and in blennorrhœa of the urinary organs. In infantile diarrhœa some regard it as specially indicated, since, besides acting as a styptic, it may serve as a nutrient. The best form of administration is either as the simple powder alone, or mixed with an equal quantity of sugar. The kind of sick-headache most likely to be benefited by the remedy is probably that occurring in persons of a hysterical temperament.—*British Medical Journal*, April 27, 1872, p. 446.

## 20.—BROMIDE OF POTASSIUM IN EPILEPSY.

DR. LEGRAND DU SAULLE has published the results of the administration of bromide of potassium in two hundred and seven cases of epilepsy. Headache, gastric disturbance, disturbance of sensation, and other troublesome symptoms, have been described as following the use of the remedy; but these he has not found to occur when the drug has been pure. When the quantity taken daily reaches 4 grammes (3 j), the reflex sensibility of the fauces, epiglottis, and root of the tongue, and the sensibility of the generative organs, are diminished. Acne then also appears; which is not, however, as has been represented, of critical importance. Dr. Legrand du Saulle commences with a gramme and a half or two grammes daily, and increases the dose gradually to 6 or 9 grammes a day; in one case, in the course of twenty-six months, the increase was to  $14\frac{1}{2}$  grammes



daily. In men, no result may be observed till the daily dose reaches 4 or 5 grammes; in women, 3 or 4½ grammes daily will sometimes produce distinct effects. Of 207 epileptic patients treated with bromide of potassium, all symptoms of epilepsy ceased in 17, who were under observation during three or four years; 28 remained free from one to two years; in 33, there was marked improvement; in 19, the intervals between the attacks were increased, and these were less severe; in 110, there was no result. Dr. Legrand considers that it is not safe to entirely omit the use of the bromide, even when there has been no epileptic attack for a year. The patients, however, must be watched; for, under prolonged use of bromide, mental disturbance, stupor, confusion of ideas, impotence, and acene, are apt to set in. The last named affection has sometimes disappeared under the use of arsenic simultaneously with the bromide of potassium.—*Gazette des Hôpitaux, and British Medical Journal, August 3, 1872, p. 126.*

## 21.—MAGNETO-ELECTRICITY IN THE TREATMENT OF MUSCULAR ATROPHY.

By DR. ALEX. P. FIDDIAN, Cardiff.

Whilst the value of electricity in the treatment of disease is a matter of dispute, a case where there is distinct evidence of its utility may be considered worthy of publication, and for this reason the following is brought forward. The patient (J. L., Bute Docks, Cardiff), to state the matter summarily, was paralysed to an extent fast approaching completeness. Both upper limbs were affected, so that he was quite helpless; yet in six weeks from the time he was first seen he was enabled to walk fairly well, and in five months more active than he had ever been. A detailed account of his condition and of the steps taken for his recovery is given as follows:—

August 19, 1871.—J. L., aged twenty-six, pilot, is a single man and a sober one. Has been much exposed in his occupation necessarily; and also, because of his eagerness to do all that came to hand, was often at work night and day. Was not subject to rheumatic pains; never had syphilis; but was always a strong, healthy, stout-hearted fellow. Strange to say, he has had an aversion to meat, and has not eaten any for years as an article of diet; he has lived almost wholly on bread. For at least a twelvemonth has been subject to constipation, and to relieve it has consumed large quantities of Epsom salts and of pills—in fact, whenever he could lay his hands on “salts” they were sure to disappear. Up to nine weeks ago he considered himself to be a picture of health, but about that time as he was walking to town he felt as if struck in the calves. In a way

unaccountable to himself, he seemed gradually to lose the power of walking—tottered, and finally ceased to walk within three weeks of the attack. His feet and legs lost their sensibility and their power. Then his thighs and forearms lost their power, but not sensibility.

He has used a great variety of applications, external and internal, but every day the disease seemed to grow upon him; and, except in the matter of sensibility, his condition has all the time steadily become worse, and the same constipation, which for the last twelve months has been a trouble, during the last nine weeks has been aggravated. His appearance is that of a strong, healthy man, but he cannot shake hands with anything like a good grasp. His first complaint is that he cannot stand, for his knees give way at once, and if taken out of bed he must be supported. He cannot lift either foot from the bed, but in trying to do so raises the knee a few inches. He cannot flex or extend either foot or move his toes. He can only turn in bed with the greatest difficulty and clumsiness. His hands, too, he complains of as quite useless, for he can neither grasp a book nor unbutton his shirt, nor hand his food to his mouth. He makes no complaint about his arms, and, indeed, he moves the forearm upon the arm or the arm at the shoulder-joint with his usual freedom and force. No reflex action is observed on tickling the soles of the feet, nor is any twitching observed in either arm or leg.

Tremulousness characterises every movement. Swelling comes in his feet and legs if he allows them to hang down; otherwise they are much like sticks. His thighs are small and flabby, especially on the inside. His hands are thin, and the bones prominent; there is decided flattening between the thumb and first finger. His forearms are small and offer a contrast to his arms. Sensation as tested by pin's point at varying distances is good everywhere.

To make the diagnosis complete, the magneto-electric machine was used, and as it was applied in the same manner as was afterwards found necessary in the treatment, a statement of the manner will not be out of place here. The machine used was the common electro-magnetic, with a single large magnet, and the strength of current was regulated roughly by drawing out the keeper. Wet sponges were placed within the conductors, and were applied to the skin in such a manner that the sponges alone touched its surface. Each superficial muscle was electrified in turn, by simply placing the wet sponges on it so as to cover as large a portion of the one muscle as possible, and the application lasted for about two minutes. By this means it was found that the muscles of the forearm, though feeble in their response, were much more active than those of the thigh and



leg, hand and foot. Those of the thigh were more active than those of the leg; the hand was better than the foot. It was difficult to perceive any movement in the latter with the most powerful current he could bear. A good idea of the feebleness of muscular power in the leg may be gathered from the fact that with all the anterior tibial muscles acting together the foot just moved.

It must also be noted that in certain situations, however much the sponges were wetted, the application was very painful. Thus, on each calf, where mustard poultices had been applied, no amount of wetting could rob the electricity of its pain-producing power, when sufficiently strong to make the muscles contract. Over the buttocks, too, where no mustard poultice had been applied, sensibility was still more acute, and remained so the whole time, whereas that of the calves disappeared after a fortnight. The dorsum of the foot and the back of the hand were also very sensitive.

A diagnosis was then made, to the effect that the muscles were atrophied without any affection of the spinal cord or motor nerve trunks; that the disuse of the muscles was adding to their atrophy, though not the primary cause.

A prognosis on the strength of this was given to the effect that the limbs would regain their power, but that time and patience would be largely drawn upon.

A plan of treatment was also determined—namely, to electrify each muscle every day (the whole taking two hours or more) for about two minutes; to take three times a day a mixture containing three minims of liq. strychniæ for a dose, and to increase the dose gradually. (He went up to eight minims.) Also a stimulating liniment to be rubbed into the legs.

After a week the liniment was given up, because of a rash it had produced, and at the same time he was put on full meat diet. He ate heartily of beefsteak and eggs. No beer, spirit, or wine was ordered.

His progress was as follows:—

August 24. Can lift left leg to the height of one foot, and right leg to height of six inches.

26th. Able to stand with the help of one hand.

30th. Can feed himself, rise up from sitting posture, stand a moment without help, and grasp without trembling. Calves and shins filling up. Can flex and extend left foot.

September 1. Able to undo buttons of his sleeves.

5th. With his hands can lift a large quarto book.

9th. Walks from one room to another with help.

16th. Bowels moved twelve times in twenty-four hours; therefore liq. strychniæ was omitted, quantity of food reduced, and chalk mixture given.

25th. Walked downstairs and up again with help. No diarrhoea for several days past.

October 4. Able to walk with one stick, to flex each leg, to point each foot without flinging it about. Bulk of legs and of thighs increases. Without producing pain, no contraction of the extensor brevis digitorum takes place. Resumed liq. strychniæ, four-minim doses.

20th. Able to walk a short distance without stick. Walked across the road for first time. Notable increase in steadiness of limbs.

28th. By occasionally touching the wall is able to walk a considerable distance without his stick. Power of hands as great as that of many men, but not equal to his former self. Muscles of leg, thigh, buttock, and forearm of their natural size and firmness. Those of hand still show room for improvement, as well as those of the foot. There is an advantage decidedly in favour of the hand. Able not only to button his shirt at the wrist, but also at the neck, where he can have no help from his eyes. Uses both knife and fork in eating. His feet have never swelled since August 19. This was the last time electricity was applied.

November 5. With his own hand wrote to say he was gaining strength daily, and going into the country for change of air—Cheltenham.

April 7, 1872. Returned from Cheltenham, where he had taken certain baths, and rapidly gained in walking power; then took to his occupation, and pursued it more vigorously than ever, keeping to his new diet of meat. Latterly has been for a short tour in Scotland, leading all the while a most active life. No defect can be noticed of any kind.

The first question to be answered is, whether the electricity was the remedial agent? It evidently was, and for these reasons. Up to the time of its application the limbs were steadily growing worse. The patient, with a concern that deepened every day, had watched the wasting and flabbiness and weakness of his legs extend upwards to his thighs. From the time of its application they made uninterrupted progress towards recovery. It may be objected that the strychnia was as powerful an aid as the electricity, and so I might have thought had I not learned afterwards that he had been taking it previously in just as large doses as I gave him, if not larger. Further, had the improvement in his diet a material connection with his recovery? It no doubt aided materially, but was not the starting-point; for when it was adopted a marked improvement had already begun. Having watched the case most carefully, I feel convinced his recovery was due to the electrical agent. The time expended was considerable; but a point of more importance is,



whether the daily and prolonged stimulus does not produce a state of irritability. On the contrary, it turned out that what was borne with difficulty at first, was borne with ease afterwards. At first I hardly knew how to make its application mild enough; at last he could bear more force than I could command.

With regard to the cause a good deal might be said about hard work of muscle, with deficiency in meat diet, and about degenerate structure; but enough has been said to mark out the value of electricity, and its claim on our attention as a remedial agent.—*Med. Times and Gazette*, July 20, 1872, p. 66.

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## 22.—ON AUDITORY VERTIGO (MENIERE'S DISEASE).

By Dr. J. HUGHLINGS-JACKSON, Physician to the London Hospital and to the Hospital for the Epileptic and Paralysed.

There is a class of cases in which the symptoms, superficially regarded, appear to point to disease of the central nervous system, but which are really owing to affections of the ear. It is very important to bear in mind that severe paroxysmal vertigo may be entirely independent of primary changes in the nervous centres, and yet dependent on a peripheral nervous lesion. Every physician will have seen cases of deafness and tinnitus attended by vertigo and occasionally by that grouping of more severe symptoms called Ménière's disease. But I do not think we are sufficiently alert to the frequency of this association of symptoms. I must confess that I did not properly realise the extreme importance of carefully considering the state of the ear in cases of vertigo until I had read Knapp's paper. Knapp not only speaks of what I may call the "ear part" of these cases, but enters into a consideration of the whole of the symptoms in a manner which makes his observations of quite as much interest to physicians as they are to aural surgeons. He draws attention to the desirability of studying aural diseases in connexion with eye diseases. This I would strongly urge.

It is not difficult for a careless observer to pass over the associated deafness in cases of vertigo. For, strangely, patients often do not know that they are considerably deaf of *one* ear; and, stranger still, will sometimes persist that they are not, until a special examination convinces them that they are.

I will now give a sketch of the grouping of symptoms which constitutes what is called Ménière's disease.

Trötsch ("Treatise on Diseases of the Ear, translated by St. John Roosa") says of Ménière's disease—"Ménière, in the year 1861, drew attention to a series of most remarkable cases which

appeared in the form of an apoplectic congestion of the brain, with sudden vertigo, vomiting, great ringing in the ears, and a fainting condition, and which frequently left behind a certain impediment in motion, a continuing unsteadiness in standing and walking, and thus gave the surgeon from the beginning an impression of a congestive affection of the brain; but the constant recurrence of all these disturbances, and the fact that there generally was a very remarkable difficulty in hearing, for which no assignable change in the ear could be found, decidedly proved that there was an affection of the internal part of the organ"—(*op cit.*, p. 503). Ménière believed it to be most probable "that the material change which lies at the foundation of these disturbances has its seat in the semicircular canals"—(Tröltzsch, *op. cit.*). He arrived at this conclusion, in some measure, by reasoning from the well-known physiological experiments of Flourens. I shall avoid the temptation of speaking of these experiments, partly because they can be read in text-books of physiology (see especially Vulpian, who, in his "Physiology of the Nervous System," devotes a lecture to "Mouvements de Rotation"), and partly because their bearing on Ménière's disease has already been many times discussed (see, for instance, the writings of Tröltzsch, Knapp, Brunner, and Duplay).

I will, however, mention the interesting speculation of Goltz in reference to the functions of the semicircular canal, again borrowing from Knapp (*op. cit.*, p. 224):—"Czermak found that pigeons, after injury of the semicircular canals, frequently have vomiting. Goltz confirmed that the animals thus injured have vertigo, and dwells on a rational explanation of these remarkable phenomena. If the auditory nerve and its expansion in the labyrinth, he argued, were merely subservient to the sense of hearing, this could not satisfactorily account for the surprising disturbances of motility. He arrives at the conclusion that the two fascicles of the auditory nerve have different functions: the cochlear fascicle is the nerve of the special sense of hearing, but it is questionable whether the semicircular canals are also organs of audition. Being subservient to the maintenance of poise, *they are, as it were, the organ of the sense of equilibrium of the head, and thereby of the whole body.* Goltz gives a highly interesting analysis of this theory, explaining how he imagines the maintenance of equilibrium to be effected by the semicircular canals. The terminations of the nerves in the ampullæ and canals are excited by pressure or tension, like the tactile nerves of the skin. The liquid in the semicircular canals—endolymph—according to physical laws, distends most those portions of the wall which lie deepest. The pressure from the fluid varies with the movements of the head, so that



a determined nervous excitement corresponds to every position of the head. The perception and consciousness of the special nervous excitement by the brain constitute the sense of equilibrium, which serves as a regulator of the movements. If a portion of the semicircular canals is injured, the brain receives inaccurate information of the position of the head, and is unable to calculate and direct its movements correctly. This is the cause of the vertigo and the disturbance of motility." Goltz thought that if the lesion of the semicircular canals is of but one side the troubles will be only temporary, but if of both sides they will be permanent—(Duplay, *op. cit.*).

Even if it be granted that the semicircular canals are the parts in fault in Ménière's disease, they may be only *secondarily* so; they may be, indeed, only *suffering* from some fault in the more external parts of the aural apparatus. Increased pressure (resulting from stoppage of the meatus by wax, by catarrh, acute or sub-acute, and purulent processes in the cavity of the tympanum), "transferred from the stapes to the vestibulum, must necessarily place the semicircular canals in an abnormal state of pathological irritation"—(Tröltsch, *op. cit.*). In other words, the *primary* changes may be in the non-nervous part of the ear. It is only in suddenly occurring cases that we can suppose symptoms of Ménière's disease to depend on primary changes in the labyrinth itself. Politzer (quoted and indorsed by Tröltsch, *op. cit.*) says—"If a person who has formerly heard well *becomes suddenly* deaf, or hard of hearing, with the symptoms of an apoplectic attack, and there is at the same time an uncertain and staggering gait, but there are no symptoms of paralysis in other nerve tracts, and if the examination shows a normal membrana tympani and perfectly permeable Eustachian tube, we may believe, *with great probability*, that there is an affection of the labyrinth"—(Tröltsch, *op. cit.*, p. 507). (No italics in the original.)

Further, Knapp (*op. cit.*, p. 236), who has reconsidered the whole subject in a most able and original manner, believes that the pathological changes in Ménière's disease are *not confined to the semicircular canals*. The accompanying deafness, he thinks, shows that the cochlea is also involved. There is sometimes, he has pointed out, deafness for certain groups of tones. "It is a *contraction of the range or field of audition*, as we have contractions of the field of vision. The latter is observed in different eye diseases, but the kind found in *glaucoma* most resembles the contraction of the range of audition." He says, also—"Deafness for certain groups of musical sounds is *proof positive* that the disease is *neither limited to the acoustic nerve outside the Labyrinth nor to the semicircular canals and vestibule*, but that it *extends to the Cochlea*" (capitals in original). It is admitted by

most physiologists that the perception of musical sounds depends on the cochlea and probably on the fibres in Corti. "Helmholtz believes that the acoustic expansion of the vestibule and in the ampullæ serves for the perception of the non-periodical or irregular vibrations—that is, for ordinary sound but that Corti's fibres in the cochlea are designed for the perception of the periodical movements of the air—that is, for the musical tones. According to Helmholtz, the tuning (*Stimmung*) of the latter is different, and corresponds to the regular gradation of the musical scale. The perception of different heights of tone is therefore a perception or sensation in different nerve-filaments. In this view, the different quality of the auditory sensations or perceptions as to the height of tone and sound-colour (*Klangfarbe*) must be referred to the variety of the nerve-fibres which is excited."—(Tröltsch, *op. cit.*, p. 509).

I will now conclude these remarks—or rather, I ought to say, this compilation—by the following extract from Tröltsch:—"One of the most valuable contributions to the science of nervous deafness we owe to late French investigators, especially to the late Dr. P. Ménière, of the Paris Deaf and Dumb Institute, who was altogether one of the most meritorious workers in the province of aural surgery."—*Medical Times and Gazette*, August 17, 1872, p. 169.

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#### DISEASES OF THE ORGANS OF CIRCULATION.

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### 23.—THE RELATIVE FREQUENCY OF DISEASE BETWEEN THE RIGHT AND LEFT SIDES OF THE HEART.

By Dr. CORNELIUS BLACK, Corresponding Fellow of the Imperial Society of Physicians, Vienna, &c.

If the question were asked, "Which side of the heart is the more frequently affected by disease?" the answer in perhaps nine cases out of ten would be that the left side of the heart is the one which more frequently suffers. This answer would not, however, embrace the whole truth. It would be true of the aggregate of cases of cardiac disease without reference to age; but it would be untrue if the occurrence of cardiac disease were referred to the later periods of life. If a man lives to the age of forty years without having suffered from cardiac disease, and if after that period the heart becomes affected, the mischief will, as a rule, be found to exist on the *right* side. If, on the contrary, cardiac disease should occur before that age, the disease will almost invariably be found to exist on the *left* side. Hence it follows that the right side of the heart is the seat of cardiac



disease occurring after middle age—the left side of the heart the seat of cardiac disease occurring before middle age.

As in time, so it is with respect to the nature of the diseases which affect the right and left sides of the heart respectively. Those of the right side are the result of tissue-degeneration, or of mere mechanical influences; those of the left side are almost invariably the product of inflammation. The former are diseases which tend to widen the valvular apertures and to dilate the right side of the heart; the latter are diseases which tend to contract the valvular apertures and to increase the size and bulk of the left side of the heart.

Disease of the right side of the heart is essentially passive and secondary in its character; disease of the left side of the heart is essentially active and primary in its character. I speak now of disease when it occurs, not when it has existed for some time. Active inflammation of the left chambers of the heart arises; it progresses to a certain extent; treatment subdues it; the patient recovers; but a certain amount of damage is left behind. Years pass on; the patient during this time appears none the worse for his previous illness; but at length pulmonary symptoms suddenly manifest themselves, and then it is that the physician discovers that the left side of the heart is permanently damaged, and that the present condition of the lungs is traceable to this cause.

In this instance the mischief in the heart inducing this condition of the lungs is not, strictly speaking, active. The first step of the cardiac disease was active; but the second step was chronic. Bit by bit—increment by increment,—after the patient's apparent recovery from the primary attack, is the valvular lesion left by such attack added to, not perhaps constantly, but intermittingly, until at length the aggregate increments of addition so hamper, oppress, obstruct, and distort the mitral and aortic valves, that secondary consequences begin to follow.

In such a case the cardiac disease producing the first degree of valvular lesion was *active*, or *acute*; whilst that which really induced the secondary consequences—congestion of the lungs, hæmoptysis, hypertrophy of the lower lobes, or hypertrophy of the left ventricle—was essentially chronic.

Why are the affections of the two sides of the heart essentially different in their nature? Why do those of the left side of the heart point to an inflammatory origin; those of the right side of the heart, with but few exceptions, to a non-inflammatory origin? There must be some cause for this difference. What is it? The reason is found in the difference which exists between the constitution of the blood which reaches the left side of the heart from the lungs and that which reaches the right side of the heart from the general system. The blood reaching the left

side of the heart from the lungs has been replenished with all the elements necessary for the growth of the tissues; it has been purified, renovated, and vivified by its oxygenation in the lungs, and it is thus rendered in the highest degree stimulating to the left heart. The blood reaching the right side of the heart from the general system has been deprived, by the requirements of growth, of the chief portion of its nutrient materials; it has been fouled by the *dèbris* of tissue-waste; it has been further poisoned by its impregnation with carbonic-acid gas; it is therefore a depressant, rather than a healthy excitant, to the right heart. True, it brings with it to the chambers of the right heart the products of the digestion of food; but what are they, either as nutrients or excitants, when they reach that point? They are no more than inert, unusable, passive elements. Not until they have passed to the lungs, and have there received the vivifying influence of oxygen, can they enter into the real composition of the blood, and thus become active, exciting, disposable constituents of it.

“Like begets like” in very many instances. This axiom is true in relation to diseases of the heart. The rich, stimulating blood of the left ventricle urges, feeds, and actively supports any disease which may arise at that point; whilst the poor, impoverished, fouled, tainted, and attenuated blood which flows through the cavities of the right heart favours disease of a correspondingly low and degenerate character.

So long as the body is rapidly built up and as rapidly pulled down, disease of the left heart maintains an active character; but when the balance between nutrition and waste is destroyed—when nutrition becomes less active, whilst waste remains the same, or is more active than before,—disease of the left heart loses more and more of its active character, and approximates more and more in its nature to disease of the right heart. In many this change begins at the age of forty; in others, not until five or ten years after that period. Thenceforward the tendency to inflammatory disease of the left heart declines—the tendency to degeneration increases. With the gradual declination of the one tendency and the gradual increase of the other, a period is at length reached when active inflammatory disease ceases, as a rule, to affect the left heart. At this juncture the left and right sides of the heart, hitherto dissimilar in their tendencies, are in this respect almost as one. The active tendency of early life has given place to the passive tendency of advancing years—inflammation to degeneration.

Acute rheumatism—a fruitful cause of cardiac disease in the earlier periods of life—is seldom seen beyond the age of fifty. I have, however, attended a case of acute articular rheumatism at the age of seventy-five; but such an instance was an excep-



tion to the rule. After fifty, acute rheumatism gives place to a form of rheumatism which slowly produces rigidity of the coats of the bloodvessels, hardens and contracts the tendons, thickens and renders stiff and rigid the ligaments of the joints, hardens and lessens the articular cartilages.

Thus, then, according to a law of nature, the *ultima linea* of life ends in—degeneration.

Apart from the influence of this law, can any accidental, casual, or avoidable circumstance favour this immutable tendency to degeneration, speaking more particularly in reference to the heart? Yes; many circumstances are daily, hourly, momentarily doing this. Thousands annually perish from heart disease, whose lives might and would have been prolonged had but proper attention been given to the simple laws of nature. These laws demand attention to the three great vital functions—the action of the brain and nervous system, respiration, and circulation.

None of these functions must be overworked, as none of them must fall short of their proper duty. Healthy, regular, daily action is their law of life. If the brain and nervous system are overworked, vitality is lowered, the resisting power of the body is diminished, disease is easily produced. If the brain and nervous system are underworked, the generation of nervous power is low and deficient, the vitality of the tissues becomes low in proportion, and disease is easily excited. Overwork exhausts, ruins, kills the body, just as the continued generation of the galvanic current exhausts the acid and wears out the zinc plate. The weakest point of the body has to bear the result of this violation of nature's laws. If the heart is that point, disease falls upon it, and death before the legitimate term of man's existence is the consequence.

To keep the body in perfect health it must be duly oxygenated. There must be free and ample interchange between the blood in the lungs and the air entering the pulmonary cells. The life-stream must be purified by its elimination of carbonic acid; it must be vivified by the absorption of oxygen. The fulfilment of these conditions demands a full, free, and constant admission of pure air into the lungs. This full, free, and constant admission of pure air cannot be obtained in badly-ventilated houses, crowded buildings, schools as at present constructed, theatres, manufactories, pits, underground railways, and the like.

When the body has reached that age at which natural decay or degeneration has begun, the absence of pure air hastens and increases the degenerative tendency. Where the heart is more prone than other organs to disease, the want of pure air falls with powerful effect upon the tissues of the right heart. Their nutrition is defective by reason of the impurity of the blood

with which they are fed, their vital force is lowered, their muscular fibre loses its tonicity, degeneration and debility take the place of active nutrition and power. If in this condition any stress is thrown upon the heart by hurried walking, by lifting, climbing, violent declamation, passional expression, singing, laughing, or by any unusual exercise of the voice, the tricuspid valve gives way, it henceforth fails to close its aperture, and the results of a back-flooding of blood upon the venous system of the body begin to follow. If none of these exciting causes occur, the continued breathing of impure air is followed by constantly progressing degeneration of the tissues of the valves and muscular structure of the right heart; they become soft and feeble, their atoms shrink; the segments of the tricuspid are at length unable to meet in their attempt to close their aperture; a small chink or slit is left between them; through this the blood finds its way into the auricle above at every contraction of the heart; and soon regurgitation is followed by the secondary consequences produced in the general system—congestion of the liver, stomach, spleen, kidneys, bowels—by hæmorrhoids, general dropsy, and occasionally by cerebral mischief.

I hold that the breathing of impure air is a fruitful source of disease of the right heart occurring after middle age. How many people ignorantly favour its occurrence by confining themselves to closely-shut, non-ventilated, hot, stifling rooms, in which the carbonic acid has accumulated to two or three per cent. of the air they respire! How many are thus destroyed by being compelled, through the exigencies of life, to pass the greater part of their time in pits and manufactories where ventilation is defective, or in which the air respired is poisoned by noxious fumes and offensive emanations from the materials undergoing the process of manufacture! How many are falling victims to the poisonous influence upon the heart of the atmosphere of an underground railway! What do these facts suggest? How are these evil results to be prevented? The simple answer is—Let the rooms in which you live be effectually ventilated by an incoming current of air filtered from all adventitious impurities, and so divided that no draught shall be felt; and by an outgoing current which shall remove from the apartments the carbonic acid, carbonic oxide, sulphurous-acid gas, sulphuretted hydrogen, and other noxious compounds, as rapidly as they are generated. Apply the same principle to public buildings, theatres, schools, manufactories, pits, and to all places in which people are accustomed to congregate.

As to underground railways, the best plan is to avoid them. True, the time passed in their polluted atmosphere is usually very short; but it is, nevertheless, sufficiently long to paralyse



occasionally the heart's action, and always, by its pollution of the blood and by its direct effect upon the nervous system, to favour degeneration of the structures of the heart.

It often occurs to a medical man to visit a patient for the first time, and to find him suffering from a dilated right heart, tricuspid incompetency, some degree of hepatic enlargement, and oedema of the lower extremities. There is no asthma, no emphysema of the lungs, no affection whatever of those organs to impede the onward flow of blood from the right ventricle, no history of cardiac affection in earlier life. The patient may for some short time have been sensible of a change in his breathing on walking rather quickly, or in mounting the stairs, or he may never have felt, or at least recognised, any such sensations. His attention was first arrested by observing that his feet and ankles were swollen, especially at night on going to bed. This sign it is which gives him the first alarm, and which causes him to seek the aid of his physician. An examination of his case detects a dilated right heart, with incompetency of the tricuspid valve. How has this condition of the heart been brought about? There is no history of previous cardiac disease; there has been no illness ushering in the present condition of things; there has never been, nor is there now, any affection of the lungs, and yet the right heart has suffered a lesion fatal to life! The answer is, that every such case has passed the age of forty, that the tissues of the right heart have entered upon the period of degeneration, and that this degeneration has, with very few exceptions, been hastened by the breathing of an impure air, either during the pursuit of ordinary occupations of life, or in the patient's own dwelling.

When the degeneration of the right heart has progressed to a certain extent, incompetency of the tricuspid valve follows either with or without the aid of an exciting cause. Hence it is easy to understand why dilatation of the right heart and tricuspid incompetency are often found to exist apart from any previous history of cardiac disease.

The third great vital function which influences the degenerative tendency of the heart is that of the circulation of the blood. To preserve the health of the tissues, the blood must not only be pure and rich in the materials of growth, but it must flow with a certain speed through all the bloodvessels. If the speed with which the blood moves is on the side of either *plus* or *minus* of the standard of health, disease will shortly arise. If it is on the side of *plus*, active disease of the heart, where that organ is the one to suffer, will follow. If on the side of *minus*, tissue degeneration will ensue. Active disease will be the consequence before middle age; degeneration after that period.

These facts teach that all violent and long-continued efforts of the body should be avoided. They hurry the heart's action to an inordinate degree, they cause it to throw the blood with great force into the extreme vessels, and, as there is almost always one organ of the body weaker than the others, the vessels of this organ become distended, and remaining distended, the organ itself becomes diseased. Running, rowing, lifting, jumping, wrestling, severe horse-exercise, cricket, football, are fruitful causes of heart disease. Those which require the breath to be suspended during their accomplishment are more fruitful causes in this respect than those which require no such suspension of the breathing. Rowing, lifting heavy weights, wrestling, and jumping do this; and of these, rowing is the most powerful for evil. At every effort made with the hands and feet, the muscles are strained to their utmost; the chest is violently fixed; no air is admitted into the lungs; blood is thrown by the goaded heart with great force into the pulmonary vessels; they become distended; they at length cannot find space for more blood; the onward current is now driven back upon the right heart; its cavities and the bloodvessels of its walls become in like manner distended; the foundation of disease is laid. Hypertrophy, hæmoptysis, inflammatory affections of the heart and lungs, are the consequences in the young; valvular incompetency, rupture of the valves or of the muscular fibres of the heart, pulmonary apoplexy, and cerebral hemorrhage, are too frequently the immediate consequences in those of more mature years.

If the flow of blood is *minus* the standard of health, the heart's walls are imperfectly nourished by reason of a deficient supply of food within a given time; the blood itself, receiving less aeration, is in consequence more impure; degeneration of the heart's walls is thus induced, if it does not already exist—hastened, if it is present.

[The relative frequency of disease between the right and left sides of the heart depends as a rule upon age. The left side suffers more before forty, and the right after that age. Diseases of the left side are the result generally of inflammation, diseases of the right of tissue degeneration.]

According to the ordinary law of life, tissue-degeneration, or natural decay, usually begins at the age of forty; with its advent, the left side of the heart becomes less prone to inflammatory disease, and approaches more and more closely in its tendencies to the diseases of the right side of the heart; and the degenerative tendency of the latter is, after this age, greatly influenced by the three great vital functions—the action of the brain and nervous system, respiration, and circulation.



It is thus shown that, to maintain the heart's structures in as healthy and sound a condition as they could possibly be maintained under the law of natural decay, these vital functions must neither be overworked nor fall short of their necessary duty—that, in fact, regular, natural action is their law of life.

I have shown the effect of impure air in promoting the degenerative tendency in the structures of the heart, and especially in those of the right side of the heart, after the age of forty. I was thus led to a passing consideration of the baneful influence produced upon the heart by badly-ventilated houses, schools, manufactories, pits, theatres, underground railways, and all places of a similar character.

To this point I return, to show the influence of an increasing quantity of carbonic acid in the air respired upon the contractility of the muscular fibres of the heart.

I take for my example the newly-hatched trout. During the winters of 1869-70 and 1870-71, I hatched some thousands of this fish, many of which I daily submitted to microscopic examination. The result of my investigations, in reference to the action of the heart and to the influence upon it of a decreasing quantity of oxygen and an increasing quantity of carbonic acid in the water in which the fish was confined, shows,—

That, on placing the fish in a glass trough containing a quantity of water, the heart is seen, under the microscope, to be affected in the following manner:—

In the first few moments of examination the venous blood, collected by the veins from the head, back, and yolk-bag (the first two of which unite to form a bulbous vessel into which the third opens), is seen to be projected with considerable force and rapidity into the upper (auricle) of the two cavities of which the heart is composed, and thence as instantaneously into the lower (ventricle) cavity, which contracts with equal rapidity, and forces the blood into the branchial artery, which conveys it to the gills. The projection of the blood into the auricle, its passage into the ventricle, and its expulsion therefrom, are but the work of an instant. As the blood enters the auricle both it and the ventricle seem to anticipate the charge of blood; but especially is this the case with the ventricle. Before the blood well touches the valve which guards the entrance from auricle to ventricle, the latter is observed to shorten its longitudinal diameter, to visibly meet, as it were, the coming charge of blood from the auricle, and to force it instantly into the branchial artery. There is no delay whatever of the blood in the auricle or ventricle. It is shot in a straight line from the vena cava through the auricle and the auricula-ventricular valve, caught by the contracting ventricle, and deflected and forced, without a moment's delay, into the branchial artery.

From these observations it was evident that the contraction of the heart was not excited by the distension of its cavities, but that it was induced by the mere impingement of the blood upon its lining membrane. In contracting the ventricle was seen to roll about one-third upon its axis, by which a portion of that part of it which was previously out of sight was brought into view. As soon as it had delivered its blood into the branchial artery, it relaxed, and increased again its longitudinal diameter, recoiling from systole with an energy and a rapidity equal to those of its contraction.

In three or four minutes the heart is observed to contract both less quickly and less energetically. A very short time after this the blood can be seen gently pouring into the auricle, and thence into the ventricle, which latter now allows itself to be about one-fourth filled before it contracts. It now expels its blood, and again dilates; but its dilatation, like its contraction, is not so instantaneous as it was when first observed under the microscope. In a short time longer the sensibility of the heart is greatly diminished, for the blood is seen to be accumulating in both the auricle and ventricle, but especially in the former, from both of which cavities it is now only partly expelled by the contraction of the heart.

At length, just before death, the blood is seen to flow from the auricle into the ventricle, thence into the branchial artery and along it, the heart being passive during this time, and only now and then at long intervals manifesting a very slight and partial contraction. During the whole of this time the blood is coagulating more and more in the auricle and ventricle, but especially in the former; and when at length the heart has ceased to beat, the auricle and the vena cava opening into it are fully distended, whilst the ventricle is only partly distended with black-red blood. In the last moments of life, after the heart has ceased to beat, the branchial artery is seen to be pushing forward its slender current of blood, and to become at length quite empty and transparent.

Here, then, as the oxygen dissolved in the water in which the fish is confined becomes exhausted, and as the carbonic acid increases, the sensibility and contractility of the heart are diminished, and at length entirely destroyed. The negation of oxygen, and the increase of carbonic acid, have culminated in the death of the fish.

*Precisely the same effect is produced upon the human heart by an accumulation of carbonic acid in the air respired.*

In the ordinary condition of the atmosphere, in which carbonic acid does not exceed one part in a thousand parts of that medium, its effects upon the heart are inappreciable. When, however, the carbonic acid has accumulated to the extent of one



per cent. of the air respired, it begins to produce a slight feeling of faintness, and some degree of uneasiness across the brow.

At 2 per cent. the heart's action is quickened, the sense of faintness is greater, there is slight giddiness, with heaviness and constriction of the head, together with nausea. At 3 per cent. all these symptoms are increased. There are vertigo, fluttering of the heart, nausea and sickness, followed by an overwhelming sense of muscular prostration. At this moment the contractions of the heart become very feeble, the skin relaxes, and is bedewed with a cool, clammy perspiration. These symptoms deepen with the increasing quantity of carbonic acid in the air respired until the utmost limit of toleration is reached, beyond which life can no longer be maintained. At this stage lethargy supervenes; and, at the moment of its occurrence, the heart begins to beat less frequently, and much less powerfully than before. This condition is the parallel of that observed in the young trout, when the blood begins to accumulate and to coagulate in the auricle and ventricle, and when the heart's sensibility and contractility are reduced to the greatest degree.

From these effects it is certain that confinement to an atmosphere impregnated with carbonic acid, even to the extent of one per cent. only, quickly deranges the function of the heart, and ultimately deteriorates the tissues themselves of that organ.

The greater the percentage of carbonic acid in the air respired, the more quickly and the more profoundly are these effects produced. The constant breathing of air containing one per cent. only of carbonic acid proves fatal to life; but if it is respired for a short time only, functional disturbance alone is then and there produced. *It is, however, certain that in this functional disturbance lie the germs of organic mischief, and that frequent repetition of the cause will undoubtedly end in organic disease. Hence the impure atmosphere of the bedrooms of the poor, and, indeed, of many of the middle class, proves a sharp spur to the degenerative tendency manifested by the heart, and especially by the right side of the heart, after the age of forty.* Such bedrooms are generally small in their superficial area, low-pitched, and often lighted by a diminutive window, which at night is kept constantly closed; and having a door which opens to the interior of the house, but which is also closed during the occupation of the rooms. Nay, to prevent the slightest admission of fresh air, the crevices of both window and door are most carefully stopped; and, to render the matter still worse, a fire is not unfrequently kept burning during the winter nights.

What must be the effect produced upon the air of such rooms under the conditions named? I take for example an average-

sized bedroom in the cottages of the poor—say, a room twelve feet long, ten feet wide, and eight feet high. This gives a cubical space of 960 feet, which is not more than half the cubical space allowed each patient in our best arranged hospitals. In this room, with its diminutive window and door constantly closed, three, four, and frequently a greater number of persons pass the night of eight or ten hours' duration. No provision has been made for the admission of fresh air—none for the escape of the carbonic acid exhaled during respiration. What little provision did exist in the crevices formed by the badly-fitting door and window has been carefully abrogated by sand-bags, rolls of rags, and stuffing of every description. Thus the air of the room becomes poisoned with carbonic acid, and in this condition it is breathed and re-breathed, to the manifest injury of the heart.

A simple calculation will enable us, if not to determine with exactitude, at least to approximate to the amount of carbonic acid exhaled by each sleeper, and consequently to the degree of vitiation which the air of the apartment undergoes. I fix the number of respirations at its minimum—14 per minute; the quantity of air exhaled at each expiration at 20 cubic inches; the quantity of carbonic acid contained in the expired air at 4 per cent.; and the duration of the night at 8 hours. Hence,  $14 \times 20 \times 8 \times 60 = 134400$  cubic inches, or 77.77 cubic feet, of air expired by each sleeper during the night of eight hours. This expired air contains 4 per cent. of carbonic acid.  $\therefore 100 : 77.77 :: 4 = 3.11$  cubic feet of carbonic acid exhaled by each sleeper during the night. Suppose the number of sleepers occupying the bedroom to be four, this will give  $3.11 \times 4 = 12.44$  cubic feet as the quantity of carbonic acid which is exhaled by the four sleepers during the eight hours of night. The room itself contains 960 cubic feet of air, and through this 12.44 cubic feet of carbonic acid would have been diffused by the termination of the night. It therefore follows that, if no fresh air entered the room, and if in consequence the carbonic acid had no means of escape, the air of the apartment would, at the end of the eight hours of night, contain 1.29 per cent. of this gas: a quantity sufficient to produce serious results.

This statement, however, does not represent all the facts of the case. It must be remembered that the oxygen contained in the air of the room would be constantly undergoing reduction by respiration during the night. If the quantity thus consumed were determined from the quantity of carbonic acid exhaled, allowing for the fact that 15 per cent. more oxygen is taken into the blood than is contained in the carbonic acid of the air expired, it will be found that from one-third to one-half of the oxygen originally contained in the air of the room would



have been consumed by the end of the night. This reduction in the quantity of oxygen, and the great increase of carbonic acid, would affect the body in two ways—firstly, by a deficiency of oxygen, and secondly, by an excess of carbonic acid, in the air respired. Hence the reduction of the one and the increase of the other would render the air far more injurious than if only one of these changes in its constitution had taken place.

The actual result is not, however, in strict accordance with this calculation, because fresh air, although in limited quantity, does find its way into the room, and carbonic acid does, to a limited extent, find its way out. These, therefore, would modify the constitution of the air of the room at the close of night; but they would still leave it with an excess of carbonic acid injurious to life.

It is found that when air moderately impregnated with carbonic acid is inspired it greatly impedes the exhalation of more from the lungs; and that the greatest quantity of carbonic acid which exists in pre-breathed air never exceeds 10 per cent. It is much to be feared that to this degree of vitiation the air of the bedrooms of the poor and of others not unfrequently rises by the too prevalent system of excluding fresh air, and by the frequent absence of provision for the escape of that which has already passed through the lungs.

Can it then be a matter of surprise that death from diseased heart should so often occur during the night?

*In thousands of instances of cardiac disease life is thus sacrificed, where had but proper ventilation of the bedrooms been observed, the subjects of such disease might, despite the cardiac mischief, have continued to live for an indefinite time.*

It has frequently been my duty, during a practice of nearly thirty years in the midst of a large community prone by the habits and particular avocations of the people to heart disease, to investigate cases of "found dead" in bed, and I have often been compelled to refer the immediate cause of death to the effect of carbonic acid liberated by respiration and confined to the apartment, in destroying the sensibility and contractility of the heart, rather than to the direct influence of the diseased heart itself.

I remember that on one occasion I was summoned to a case which had occurred in a bedroom 15 feet long, 12 feet wide, and 8 feet high. In this room, with the door and window closed, no fewer than twenty persons slept night after night! Can anyone doubt that the air of such a room would be charged to excess with carbonic acid exhaled by respiration? Those who perished in this manner were beyond the age of forty; and, in every instance examined, the right side of the heart was

either primarily affected by tissue-degeneration, or by disease consecutive to mischief in the left side of the heart and lungs.

Often indeed in the dwellings of the middle and higher classes of society the provisions for ventilating both their bedrooms and their day-rooms are miserably inadequate to preserve health. The consequence is that cardiac disease is promoted to an inconceivable extent. *There is no other disease in which the demand for cold, fresh air is so urgently pressed by the patient as in cardiac disease. There is none in which a constant supply of pure air is more needed—none in which it is more grateful to the patient, or in which it has a more immediately beneficial effect.* At all times and seasons—in the depth of winter—by day and by night—a patient suffering from a paroxysm of cardiac asthma will hurry to the open door or window, and there, with his body hanging half out, will remain, with scarcely any vestments upon him, breathing the cold air until the paroxysm has ceased. Ought not this urgent, this powerful supplication of nature to teach us the importance of ventilation, and of a full supply of fresh air in the treatment of heart disease?

*I hesitate not to say that free ventilation—the free admission of pure air into the apartment by day and by night—is one of the most important remedial measures which can be adopted in the treatment of this disease.*

Where this means is defective, but where, nevertheless, the vitiation of the air of the bedroom does not exceed one per cent. of carbonic acid, a sensible effect is produced upon those who have slept within its influence. They complain, on leaving their bed, of weakness; their limbs tremble; they feel somewhat giddy, and their head feels heavy, or it aches. The least effort disturbs the heart's action, which is somewhat quick and feeble; their countenance is pale; the lips are not unfrequently somewhat blue; and the tongue is covered with a thin, whitish, and somewhat slimy fur. The appetite is in abeyance; there is a feeling of nausea; and the first evacuation is generally dark in colour.

What is the pathological condition of such patients at this moment? Simply this. The blood contains an excess of carbonic acid, which, circulating with the blood through every organ, disturbs the natural action of every organ, blunting its sensibility, vitiating its particular function, and interfering with those molecular changes which constitute healthy nutrition.

A person thus affected does not usually die. The body, removed to a pure atmosphere, begins at once to excrete the carbonic acid by the lungs, the liver, the skin, the kidneys, and the bowels, and in the course of a few hours the more visible manifestations of its baneful effects have passed away. It



however often happens that a sense of weariness and muscular debility is felt for days afterwards. Night, too, frequently places such subjects in the same condition as before. The same bedroom is occupied; the same inadequate means of ventilation continue; the same accumulation of carbonic acid takes place; and the same effects upon the bodily organs are repeated. Blood charged with an excess of carbonic acid again pervades every tissue of the heart, diminishing its vitality, lowering its sensibility, and assimilating its nutrition to that of the reptilian heart. But the low character of the nutrition of the reptilian heart does not accord with the comparatively quick circulation, rapid nutrition, vital power, and energy of action required by the human heart. The one cannot be substituted for the other, In man the change results in disease where disease does not exist—aggravates disease where it is already present.—*Lancet*, Aug. 24 and Sept. 7, 1872, pp. 253, 329.

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DISEASES OF THE ORGANS OF RESPIRATION.

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24.—ON ASTHMA.

By Dr. C. J. B. WILLIAMS, F.R.S.

I call your attention to a group of affections which go under the title of *asthma*. This word is commonly used as synonymous with dyspnœa, or difficult breathing; but it is more convenient and practical, as well as correct, to restrict it to those kinds of difficult breathing which are accompanied by audible *wheezing*. Here is at once a great practical distinction from the dyspnœa of pleurisy, of pneumonia, of diseases of the heart, and of diseases of the blood (anæmia and toxæmia)—these are all *panting*, or *gasping*, with breath short and frequent, but with little or no wheeze. The dyspnœa of *asthma*, on the other hand, is essentially *wheezy* and *prolonged*, affecting the inspiration, or the expiration, or both. This is most distinctly witnessed in spasmodic asthma and pulmonary emphysema; but it applies more or less to severe bronchitis and bronchial catarrh, which carry with them more or less of the asthmatic wheeze.

The simplest, as well as the most characteristic variety of these, is the purely spasmodic asthma. A person subject to this may be attacked suddenly on entering a close or dusty room, on inhaling the fumes from a stable, the odour of ipecacuanha, or other smells in peculiar cases; or, more commonly, he awakes in the middle of the night with a feeling of oppression approaching to suffocation—referred by some to the throat, by others to the sternum, by others to the epigastrium—obliging

the patient to sit up in bed or in a chair, with the elbows rested on the knees, the shoulders elevated, and the head bowed forward, but all labouring to the utmost in strong prolonged efforts of inspiration and expiration. This painful struggle for breath may last from a few minutes to several days, according to the severity of the paroxysm; and frightful as it seems to witness, and distressing to the patient, yet it is not dangerous; sooner or later the tight breath is relaxed, cough and expectoration sometimes accompanying its relaxation.

Seeing how violently all the muscles of inspiration and expiration partake in the struggle of a severe paroxysm of spasmodic asthma, we cannot much wonder at the notions of older writers on the subject (Bree and others), that the disease depended on an excessive and convulsive action of all these muscles. Laennec first pointed out the true pathology of asthma in tracing it to a spasmodic contraction of the bronchial tubes, which so much impedes the ingress and egress of air in respiration as to call for excessive and violent action of the respiratory forces to effect it. In this temporary constriction of the bronchial tubes we see the immediate cause of the difficult breathing; and this, too, is the cause of its most characteristic sign—the loud wheezing, whistling, piping, and cooing sounds which attend the paroxysm.

The correctness of Laennec's view of spasmodic asthma had, however, been called in question by several writers, who opposed to it the very daring assertion that the bronchial tubes do not really possess muscularity. The supposed muscular fibres, which had been demonstrated by Reisseissen, were declared not to be muscular at all, but merely elastic, and therefore could not be the seat of spasmodic contraction. To settle this question, about twenty years ago I made a series of experiments on animals, and they proved beyond doubt not only the muscularity of the bronchial fibres, but also the kind and degree of irritability which they possess. Under the influence of a galvanic, mechanical, or chemical stimulus the circular fibres of the bronchi contract readily but slowly, and gradually relax when the stimulus is withdrawn. The contraction is more tardy than that of the œsophagus, but more prompt than that of the arteries, and relaxation does not follow for some minutes after. Like other muscular contractility, it becomes exhausted after continued stimulation, and it is recovered by a period of rest.

Although it has been generally admitted that my experiments have clearly proved the muscular contractility of the bronchial tubes, yet from the remarks of recent writers on the subject it appears to me that the physiological offices of this property have not been generally understood. Thus it has been supposed that the bronchial muscles contribute to the rhythmical process of expiration; but if they do so at all it can only be to a slight



extent, as their movement is much slower than that of ordinary respiration, besides which it appears to be limited to the larger and middle-sized tubes, and does not extend to those of smallest size. A galvanic current caused little or no contraction when passed through a lobe within an inch or two of its margin; but the contraction was marked when the current was passed along any of the tubes of the size of a small crowquill upwards. I cannot, therefore, concur in the notion propounded by Dr. Gairdner in one of his able papers, that the finest bronchi have a peristaltic or vermicular motion like that of the intestines, and that this is the chief means by which fluids are expelled from these tubes. It is probable that ciliary motion contributes to this end, but the expulsion of mucus and other fluids from the bronchial tree is mainly effected in the manner which I pointed out more than twenty years ago—by the rapidly increasing velocity with which the air in expiration passes from the pulmonary cells to the narrower converging bronchi, and carries with it any loose liquid in its way. The air in entering the lungs passes with decreasing velocity and force as it spreads into the minute tubes and cells, the combined area of which vastly exceeds that of the large branches and trunk of the bronchial tree. On its return in expiration this is reversed; the motion is more rapid and forcible as it converges towards the trachea and glottis, where the process is brought to its consummation in special efforts of expectoration and coughing. This draining operation of the expiratory act is promoted on the one hand by increasing the force of the act by muscular effort; and, on the other, by a moderate contraction of the tubes, which augments the velocity and sweeping power of the air in passing through them. This contraction is effected by the bronchial muscles, which thus assist in the process of clearing the tubes. But if these muscles act in excess, they render the passage too narrow both for free expiration and for expectoration; and this is just what occurs in the asthmatic spasm, which renders these processes slow and difficult. This is the view of the function of the bronchial muscles which my experiments led me to entertain, and which I published and taught in my lectures more than twenty years since. Dr. Hyde Salter, in his late able and elaborate work on asthma, proposes a like opinion, and suggests that the bronchial muscles may also be useful in impeding the entrance of irritating matters into the lungs.

But the most interesting fact discovered by my experiments, with regard to this bronchial contractility, was, that it is influenced differently from that of other muscles by various poisonous or medicinal agents. This hydrocyanic acid did not impair it at all, opium and morphia very little, conium and aconite a little more, but belladonna and stramonium almost destroyed

it; so that in animals poisoned by these drugs the bronchi showed scarcely any signs of contractility when stimulated. In animals poisoned with strychnia the bronchi seemed permanently contracted, so that a stimulus had no further effect. This is another proof of the antagonistic action of strychnia and belladonna. Now, it is well worthy of remark that the action of these several drugs on the bronchial tubes is quite different from what it is on the œsophagus and intestinal canal. Thus, in animals poisoned with belladonna the œsophagus was as irritable as ever; in those poisoned with opium, on the other hand, the contractility of the alimentary canal is much impaired. This fact has led me to give belladonna in preference to opium in cases of intestinal colic with constipation, and with a far more satisfactory result. I think this mode of investigation worthy of attention as a means of extending pathological and therapeutical knowledge. We want a more elementary study of the operation of medicines, an examination of the effect of simple drugs on the functions of elementary tissues, and on the constituents of the blood; for without this we cannot hope to understand the operation of complex medicines on the whole frame.

To return to the pathology of spasmodic asthma; there is, then, no reason to doubt that a continued contraction or tonic spasm of the muscular fibres of the bronchi—constriction of these tubes—is the essential characteristic of this disease. No doubt the contraction of the bronchial, as of other muscles, is controlled by nervous influence; and many of the phenomena of spasmodic asthma exemplify the reflex action of the nervous system in exciting the spasm—for example, mental agitation or irritation of the stomach by indigestible matter will often bring on a fit of asthma—but the nervous element is often not obvious in the clinical observation of the disease. Thus, spasmodic asthma often affects persons, not otherwise nervous, when they inhale peculiar smells or when they catch cold; and in my experiments I found that irritating the eighth nerves had little effect on the contraction of the bronchial muscles, which, on the contrary, readily answered to direct irritation. But it is open to further inquiry whether, like the heart and arteries, these contractile fibres are not more influenced through the sympathetic than through the spinal nerves.

However induced, this constriction of the tubes renders the ingress and egress of air to and from the lungs difficult and noisy, so that the process of breathing becomes very laborious and prolonged; all the muscles of inspiration, ordinary and supplementary, are called into violent action, and so the struggle goes on until the spasm is relaxed. If this takes place soon the attack ceases, and there is no remaining disorder; but if the



spasm lasts long other disorder ensues in the bronchial membrane and other parts concerned. They become congested from the imperfect and laborious breathing, and this congestion causes increased and disordered secretion; hence cough and expectoration of a catarrhal character commonly accompany or follow a prolonged fit of asthma. So, likewise, if these asthmatic attacks recur frequently, they tend to produce more permanent congestion and thickening of the air-passages, which continue in the intervals between the attacks, and thus the disease passes from the paroxysmal or intermittent asthma into a more habitual or constant asthma, with which is associated more or less of the general dilatation of the air-cells, called by Laennec emphysema of the lungs. I will allude to this again afterwards.

Spasmodic asthma, or bronchial spasm, may originate like a common cold or bronchial catarrh, or from exposure to the effluvia of a hayfield, of a stable, of a close room, of ipecacuanha, or of other dust; or it may arise from some unknown impurity or peculiarity in the air of certain places; or it may be induced by indigestion, by gouty or other irritating matters in the blood, or by mental emotion. The kind known under the term of "hay-asthma" is very common, and I have no doubt is excited by pollen-germs, or some effluvium from flowering grass and other vegetables. It is more distinctly catarrhal in nature, being often preceded or accompanied by sneezing, coryza, and other symptoms of membranous irritation. But in whatever way asthma may have its origin, when it has once occurred it is very apt to return again, and may be re-excited by any of the causes just mentioned; and, generally speaking, the more frequently it recurs, the longer and more severe are the attacks, and the more likely to leave the breathing embarrassed in the intervals.

But you may well ask, further, "What is the peculiarity that makes persons asthmatic?"—that is to say, that makes the causes above-mentioned excite in them contraction of the bronchial muscles, instead of causing the more common effects resulting from the operation of these causes on persons in general. We may reply, "A peculiar irritability of these muscles or of the nerves exciting them." But this is only an expression of the fact, and does not explain why these muscles should be more irritable in asthmatic persons than in others. This subject requires more investigation. I do not feel that I can answer it fully, so as to include all instances; but I think that I have traced in several cases signs of a slight structural peculiarity which may give to the bronchial muscles an unusual liability to spasm. This may be designated under the general term of *a slight induration or thickening at or near the root of the lungs*. In a considerable number of those subject to attacks of

asthma, even at an early period, when they are few and far between, I have found, in the absence of the paroxysms, more or less of whiffling or tubular sound in one or both inter-scapular spaces, generally most in expiration. This may be from enlarged bronchial glands, which are of common occurrence in children, often accompanying enlarged tonsils; or it may be from interstitial deposit under the mucous membrane at or below the bifurcation of the trachea; or it may be from slight partial induration of the pulmonary texture, such as that resulting from an old tuberculous lesion, whether in the form of dwindled and contracted tubercle or calcified induration, but of very limited extent. Either of these lesions, trifling as they may be in extent, may increase their irritability of the bronchial tubes, not only by mechanical irritation, but by partially obstructing the circulation underneath the tubes, and thus throwing more blood to the muscular fibres and mucous membrane. It is in favour of this view of the pathological cause of the asthmatic spasm that such changes to an increased extent certainly do occur after often-repeated and severe attacks, and operate as the causes of their continuance and aggravation. In cases of confirmed and habitual asthma we have abundant evidence, both during life and after death, of the increased vascularity and thickening of the mucous membrane, and subjacent tissues, which changes we shall afterwards find to be concerned in producing the general emphysema of the lungs which usually supervenes.

Spasmodic asthma is of common occurrence in childhood, and is then frequently complicated with eczematous and other eruptions on the skin. So, likewise, in adult life it often occurs in connexion with gout and with psoriasis, all being dependent on a morbid material in the blood; and this point should be kept in view in the treatment.

Few complaints vary in their tractability more than asthma. Some (even severe cases) yield to treatment so promptly and permanently that their cures may be ranked among the great successes of medicine.

From what has been said, it may be inferred that the treatment of asthma must vary much in its simplicity and success according to the unity or complication of the disease. Against the bronchial spasm we have remedies which are pretty effectual in most cases. Belladonna and stramonium rarely fail to relieve the bronchial spasm; and in transient cases, where this is the only element, they may suffice to cure the disease. The extracts are the most reliable preparations, and may be given in doses of from a quarter of a grain to half a grain every three, four, or six hours whilst the tendency to spasm lasts. The dryness of the throat which both these drugs often cause may be



counteracted by frequently sipping linseed-tea or barley-water. Sometimes, however, this dryness is useful in moderating the catarrhal flux which may follow the spasm.

But in most cases there exists something more than the mere spasm; and, therefore we commonly have to give these anti-spasmodics in combination with other remedies. Thus, often there is an inflammatory cold, calling for the addition of salines and counter-irritation; and this may amount to bronchitis, requiring the aid of small doses of tartarised antimony. In chronic cases, when the attacks have recurred frequently or lasted long, there is no combination more beneficial than that of iodide of potassium, in two- or three-grain doses, and ten or fifteen grains of bicarbonate of potass, with the stramonium or belladonna. I believe that I speak within bounds when I say that, with a combination of this kind, I have cured or greatly relieved hundreds of cases of asthma. The efficacy of the alkaline iodide probably depends on its eliminative and deobstruent action, increasing the secretion of the kidneys and of the bronchial membrane, and promoting the absorption or dispersion of the thickenings and deposits in the tubes, bronchial glands, and at the root of the lungs, which I have mentioned as being often concerned in exciting or perpetuating the attacks of asthma. The diuretic or eliminative action of these medicines may be advantageously increased in some cases by the addition of squill, colchicum, or tincture of cantharides, particularly where there are indications of gout or of disease of the skin. On a similar principle, in chronic cases certain mineral waters are sometimes useful, particularly those of Eauxbonnes and Cauterets in the Pyrenees, Vichy, and Ems.

There are several other remedies for asthma in common use—generally much inferior in efficacy to the preceding, but occasionally useful as subsidiary aids, and sometimes they are our chief resources where those disagree. Such is the ethereal tincture of lobelia, which in doses of from twenty to sixty drops I have known in a few instances quite successful; more frequently it has failed, and sometimes caused much nausea and discomfort. Indian hemp, in doses of a grain of the extract, gave signal relief in two instances, where the usual remedies had disagreed; but in other cases it has quite failed, and has sometimes caused distressing disturbance of the brain and heart. Smoking cigarettes of stramonium, or of the *datura tatula*, inhaling chloroform (which for safety should be mixed with sulphuric ether and alcohol), and breathing the fumes of burning nitre-paper, are expedients which often give relief in individual cases; and although this relief is less complete and permanent than that following the use of the remedies first recommended, yet they may be useful where these fail, and,

being prompt in operation, may be employed to ward off slight attacks where stronger agents are not required, or before the latter can be brought into effective operation.

Rarely we meet with cases of asthma so severe and obstinate as to resist all medicinal remedies; or it may be that the patient becomes tired of taking medicines, and renounces them in disgust—nay, sometimes I have known the symptoms aggravated by those which are commonly the most successful. In some of these change of air has succeeded wonderfully, and this not always when the change has been of the most salubrious character. In fact, the caprices of asthma with regard to air are very curious, and can hardly be accounted for. In most instances, however, a dry atmosphere agrees better than a damp one, and the air of a large town better than that of the country, especially if this be low and damp. Of places in which I have known asthmatics most free from attacks I would mention London (several parts of the West-end), Tunbridge Wells, Clifton, Brighton, and Margate (in summer); abroad, Paris, Pau, and Hyères. But asthmatic subjects should try for themselves, and remain as much as possible in the locality that they find by experience to best suit them. In the case of hay-asthma, the avoidance of the country during the hay-making season is necessary with many individuals, and the change found commonly to answer best is either to London or the seaside.—*Medical Times and Gazette*, June 8, 1872, p. 649.

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## 25.—ON VESICULAR EMPHYSEMA OF THE LUNGS, AND HABITUAL ASTHMA.

By Dr. CHARLES J. B. WILLIAMS, F.R.S.

The effect of extensive and long-continued emphysema on the blood and its vessels is manifest in the leaden pallor of the surface, contrasted with the dark congested state of the mouth, fauces, and larynx; the right cavities of the heart become enlarged; the liver, kidneys, and other organs partake in the livid congestion, and their degeneration not uncommonly follows; the arcus senilis generally appears early; and albuminuria, anasarca, and general dropsy usually precede death.

But it must not be supposed that these extreme results occur in all cases of emphysema. In more moderate degrees emphysema occurs in connexion with chronic bronchitis, and with the slight pulmonary congestion accompanying organic diseases of the heart, and, merely adding an habitual shortness of breath and wheezy cough to these affections, may be regarded rather as an infirmity than as a very serious disease. The function of breathing and circulation in such individuals is reduced to a reptile standard; and if they can be contented to remain within



their limited sphere of activity, slowly and quietly creeping their way, and not attempting in any sense to bustle, to run, or to hurry, they may live for many years in tolerable comfort.

It may be inferred, therefore, that the successes in the treatment of confirmed emphysema or chronic asthma will be chiefly of the minor class, amounting to mitigation or alleviation rather than perfect cure, and failures are to be expected in those intense and aggravated cases, in which the distension of the lung is so great and lasting as to cause much wasting and degeneration of its tissues. But there is a period in which the emphysematous distension of the lung is removable. If the causes which produce the incarceration of air in the cells (shortening and partial obstruction of the air-tubes, and thickening of the lung around them) can be taken away before the structure of the air-cells is altered by their continued distension, the lung may return to its natural condition and dimensions. Something of this kind commonly takes place after extensive bronchitis, in which there is more or less temporary emphysema. Still more does it occur after severe fits of spasmodic asthma, which induce more or less temporary emphysema; but the cause being transient, the lungs recover their normal dimensions. Now, it can be well understood that between these temporary varieties of emphysema and the confirmed and advanced disease there are intermediate degrees which may be removed or ameliorated by appropriate treatment.

Among the remedies chiefly useful are those employed as expectorants of the more stimulating class—squill, salts of ammonia, ammoniacum, and other gum resins; benzoin, tolu, and other balsamics; creasote, and other terebinthines. Some of these exhibited through the stomach, others by way of inhalation with the vapour of hot water or in spray, tend to excite the secretion and expectoration of bronchial mucus, and thereby to diminish the obstruction to the egress of air from the distended cells. Often their operation may be aided by the iodides and bromides of potassium and ammonium and alkalis, before mentioned as useful in recurring asthma. The employment of stimulating expectorants in bronchitis and emphysema requires careful judgment and discrimination; for if given too soon, or during any inflammatory or febrile exacerbation or fresh spasm, they do harm instead of good. In fact their operation is always the opposite of that of the remedies for recent bronchitis or spasmodic asthma (salines, antimonials, belladonna, &c.), and it becomes beneficial in consequence of the bronchial muscles and membrane passing into somewhat of an opposite state—inflammatory irritation having given place to passive congestion, and spasmodic contraction to atonic or

paralytic relaxation. So, too, with this change belladonna, stramonium, and salines fail to give relief, and are even hurtful. And yet the transition from one condition to another is neither constant nor uniform; and it often happens that at one time of the day there may be distressing cough, with difficult expectoration and the long crepitant respiration of an atonic state of the tubes; and at another time the tight wheezy breathing, inspiration and expiration, of spasmodic asthma. Nay, sometimes these opposite conditions may co-exist in different parts of the same lungs, and entirely baffle our efforts to counteract them.

The most common case, however, of these opposing conditions is that the atonic state prevails throughout the day, and more or less of the spasm recurs at night; and therefore it often answers well to give stimulant expectorants and even tonics during the day, and a dose or two of antispasmodic or narcotic in the night.

I have just named tonics, and they require a little further notice with regard to their influence on these diseases. Like other affections which impair sleep and appetite, and exhaust strength, continued asthma or bronchitis wastes flesh and blood; and therefore ordinary reparative tonics, such as iron and quinine, may be often useful to sustain the failing general strength and structure of the frame. But certain tonics seem to have an invigorating influence of the muscles of respiration, external and internal, and may to a certain extent restore the breathing powers, when these have been impaired by prolonged attacks of dyspnoea and cough. I allude especially to strychnia, zinc, and arsenic. In several instances the continued use of strychnia (in doses of from 1-32nd to 1-24th of a grain three times daily, dissolved in an acid) has been followed by marked improvement in the breathing powers, and a diminished tendency to the attacks of asthma. But if exhibited during attacks of spasmodic asthma, it seems rather to aggravate it than otherwise; and it is when these are over, and a state of exhaustion or atony follows, that this remedy proves beneficial. The same remark applies to the preparations of zinc, whether oxide, sulphate, or valerianate. In connexion with this operation of strychnia we may bear in mind its power to prevent vomiting and restore tone to the stomach in cases of nervous exhaustion from abuse of stimulants, or from hysteria, in which I have often found it the most efficacious of all remedies. Whether in all these cases it acts directly on the affected tissues, or through the medium of the nerves, we are hardly yet in a position to determine. Arsenic is another remedy which seems to improve the breathing powers in some cases of emphysema, and to diminish the frequency of attacks of asthma; but it is slow in its operation in the doses in which it may be safely



given (three or four drops of arsenical solution twice daily), and patients will not often persevere with it long enough to derive benefit from its use. I have found it peculiarly efficacious in the not uncommon cases of asthma complicated with psoriasis or eczema ; but its beneficial influence is not limited to these.

Asthmatic subjects are extremely sensitive to atmospheric changes, and to all influences which affect the purity and other qualities of the air. A person afflicted with extensive and confirmed emphysema, especially that of the tense kind, may be thrown into a state of severe suffering by a change in the wind, by the coming on of a fog, by impurities in the air, or a rise in its temperature, such as would not affect a person in health. It is only by avoiding all such deviations from the most salubrious standard that a patient thus affected can enjoy anything like ease and comfort, or indeed can hope to prolong life ; and fortunate are those who can afford to migrate as the seasons change, and enjoy the pure air of a dry heath in the summer, and in the winter the equally pure but tempered air of the Nubian desert. Those who cannot afford to change their residence with the season should fix their abode in a sheltered spot, on as dry a soil, and in as pure an air, as they can find—in the summer much out of doors, strolling and basking in the sun ; in winter, for the most part, shut up in well-ventilated, warm, but not overheated rooms. Such cases might be expected to derive much advantage from any means of increasing the oxygen in the air which they breathe, either by condensing the air by a forcing-pump in an air-tight room for the purpose, as has been practised at Montpellier, and other places in France, and at Ben Rhydding in Yorkshire, or by the occasional inhalation of oxygen gas more or less diluted with common air. I have known several instances in which these means have been tried by asthmatic and emphysematous patients, and some have experienced considerable relief for a time, but not of sufficient extent or permanency to induce them to persevere long in the measures, which have many inconveniences, and are not always unattended with risk. Recently, oxygenated drinks have been recommended with the object of supplementing through the stomach what the lungs cannot receive ; but it is very doubtful that either the so-called oxygen water or the diluted peroxide of hydrogen can introduce into the stomach a sufficient amount of oxygen to produce a perceptible effect in this way. But we have in nitric acid and chlorate of potass materials rich in oxygen, and readily parting with it, which may be given pretty freely without inconvenience. Twenty or thirty drops of the diluted acid with ten or fifteen grains of the chlorate in three or four tablespoonfuls of water may be taken three or

four times daily. I have often prescribed this compound in cases of emphysema and other diseases in which the breathing powers were much impaired, and in some instances with apparent benefit. From some hospital trials formerly made, it would appear that neither the chlorate nor the nitric acid can be detected in the urine. Therefore the oxygen is retained somewhere in the system; but further experiments are required to determine this point.—*Medical Times and Gazette*, June 22, 1872, p. 705.

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## 26.—ON CROUP.

By Dr. ROBERT C. R. JORDAN, Assistant Physician to the Children's Hospital, and Professor of Diseases of Children at Queen's College, Birmingham.

In all my own early teaching it was so strongly impressed upon me that "croup" was always a membranous exudation in the larynx or trachea, that it became to my mind a great difficulty to throw off the trammels of this old belief, and it was long before I could feel fully persuaded of what I now know to be the truth—namely, that the majority of the cases usually called by this name have no false membrane formed at all, but that their essential nature is an inflammation of the mucous membrane of the larynx and trachea, accompanied with secretion of tenacious mucus, and also considerable swelling caused by effusion into their submucous areolar tissue. They are in fact, catarrhal inflammation of the larynx and trachea. All other cases where exudation is really present are diphtheria; and it is in this sense, and with this definition only, that we can regard croup and diphtheria as two distinct diseases. To make my meaning clear, let me follow out the course of the two, and lay before you in general terms the broad distinctions between them, beginning with the disease which in my younger years was most familiar to me, because during times of cold or east wind it occurs sporadically in country practice all over the kingdom, and to which I was taught to give the name of croup. The early symptoms are very similar to those of an approaching attack of measles, save that there is no superabundant secretion from the lachrymal glands, and consequently no running from the eyes and nose, but there is fever and dry barking cough of that peculiar character which of necessity occurs when the rima glottidis is narrowed. This cough is indeed the crucial symptom, and yet it has probably existed for two or three days before its nature has been marked enough to alarm the mother. On questioning her you are likely enough to find that the child "has had a cold for a few days, but that she has thought nothing of it." To trained ears, however, the



peculiar cough is very manifest almost from the commencement, and if the child be asked to draw a deep breath the stridulous sound completes the diagnosis. The cough and the inspiration are both pathognomonic of croup, but of croup in its abstract sense only; they tell you that there is narrowing of the passage which allows the entrance and exit of air to the lungs—that is, of larynx or trachea. The history of the case supplies the other evidence. It is acute, and this eliminates all the chronic forms of laryngeal disease to which children are prone, such as, for example, warty or other growths. A careful examination shows no exudation on the throat or fauces, and the history is not that of sore throat but of cough; this makes diphtheria at least improbable. The symptoms have been those which would naturally take place in an attack of bronchitis and tracheitis, with a tendency to spread still further upwards and involve the larynx in the mischief; and it is the swelling of its mucous membrane, and the consequent narrowing of the chink at the outlet of the larynx, and the swelling of the lining of the vocal cords, that gives the more decided “croupy” character to the cough, to the inspiration, and to the voice, this last being often almost absent. It is these also that give the danger to the attack; the same amount of congestion, the same amount of effusion into the sub-mucous areolar tissue elsewhere would be of no serious import. Hence the mother until closely questioned dates the attack from the commencement of the danger, and says that it came on “quite suddenly;” and in truth, when laryngeal symptoms do begin, they increase very rapidly, and every hour makes the danger greater—almost every breath is more and more difficult, respiration becomes more and more diaphragmatic, the sternum is drawn in with every breath, and in spite of the increased labour less and less air passes through the laryngeal opening; the lips and face become more purple, the lungs become congested, this further increases the dyspnoea, and the child dies suffocated. Such is the natural unchecked end of the disease—a termination which it is difficult to avoid in the cottages of the poor, where from the very nature of the case, the child cannot get proper treatment, where there is no skilled nurse to follow out the doctor’s advice, and where the temperature of the room varies with every opening door. Now, what are the post-mortem appearances? There is no false membrane in either larynx or trachea, but simply a swollen and congested state of their mucous membrane, which is generally spread over with a tenacious mucus nearly as viscid as pneumonic sputa. These changes extend more or less into the bronchi, and with a congested condition of the lungs, are the only signs visible to account for death. I have many carefully noted records of such post-mortem appearances written in old

days, when the influence of what Bacon would call the "idol of the theatre" was so strong upon me that it is always stated, "false membrane in a perfectly diffuent state spread over the mucous surface." Now, this "diffuent false membrane" is, in reality, only a synonym for tenacious mucus. This is the disease which occurs sporadically in town and country alike, and which is commonly called "croup." The cause is generally exposure to cold, though some children are more predisposed to it than others; and I have known many who have had several well-marked attacks, which for the most part decrease in violence as the child becomes older. It is also a decided fact that there is a clear predisposition to it in some families, though when a child is said to be "subject to croup" it is laryngismus that is most often meant. The essence of this disease is therefore laryngitis and tracheitis of a catarrhal character, and the danger is because the entrance and exit of air to and from the lungs is impeded; the object of treatment is therefore to make a decided and quick impression on the disease. Time does not admit of the least delay. You must at once place your patient in the best possible state for recovery—that is, let him be in a warm room with no draughts, and a uniform temperature of at least 70° Fahr., and let the air which he breathes be thoroughly saturated with moisture; a boiling kettle pouring out its steam into the room often manages this very efficiently. The plan which is adopted in the Children's Hospital here is to boil a large iron kettle, to the spout of which is affixed a long tube ending in a rose like a watering-pot, from which the steam pours out copiously; but as this cannot always be at hand, your ingenuity must be taxed to find a substitute—but remember that the soft moist vapour acting locally on the swollen mucous surface is as important an agent in the treatment as any other therapeutic means. A linseed poultice to the throat helps also in this, and has certainly a soothing power. These external appliances being completed, then give at once an emetic of ipecacuanha, and repeat this every twenty minutes or half-hour, until not only copious vomiting but copious perspiration is induced. As a result of this the secretion of the air-passages also becomes thinner and more easily got rid of, and it will be borne in mind that the cough becoming looser is an excellent symptom. Increased mucous râle, without the power of cough, has of course a different meaning, but a looser cough always bespeaks a lessened danger. In addition to the ipecacuanha, a very good prescription is a powder with calomel gr.  $\frac{1}{4}$ , compound ipecacuanha powder gr.  $\frac{1}{4}$ , and chlorate of potash gr. iij., every half-hour or hour according to the severity of the symptoms. Of course the dose must be modified slightly according to the age of the patient. If the disease does not abate, next



comes the question of tracheotomy; and in this case I would leave it as long as can be done consistently with safety. So frequent is the recovery, even when the case is seemingly hopeless, that tracheotomy may be fairly called a last resort. These attacks of laryngeal catarrh can fortunately be pointed out as marked examples of the efficacy of medicine in acute disease. They are amongst the few cases where the effects of remedies can be seen, and where we can say that, if left to nature, the tendency is rather to death than to recovery. It is not meant to undervalue tracheotomy in these cases: no patient should be allowed to die without the chance which it affords. Yet so marked are the effects of curative agents that time should always be allowed for a full and decided trial of their power before resorting to the operation.

The following case illustrates this catarrhal croup forcibly:—Early in the morning of August 14, 1867, I was called to Arthur B., a little boy aged two years eleven months. I found him breathing a hundred times in the minute, the croupal sound very loud, the distress and agitation very great; the pulse could not be counted, the face was commencing to be dusky, the skin was hot, dry, and burning. The operation of tracheotomy was proposed, but it was decided to try other means first. The room was therefore made warm with the steam of boiling water, the throat painted with liquor epispasticus, and a linseed poultice applied over it. A teaspoonful of ipecacuanha wine was ordered to be taken every twenty minutes, and half a grain of calomel every hour. A little before midday a slight improvement was noted, although there had been very frequent sickness, yet the pulse was stronger, the distress less, and the labour of breathing also lessened; but by the early evening the improvement was much more decided—the respirations were reduced to twenty-five, and though a loud croupal sound was audible with every respiration yet there were moist râles also heard, and the skin was warm, damp, and freely perspiring. The pulse was only 90, and from this time the process of recovery went on steadily. It is seldom that a case presents such a decided improvement as this in so brief a time, yet the period of extreme danger is always short. To the treatment used I would now make one exception viz., the blister. Had tracheotomy been necessary, as indeed at first seemed most probable, the blister would have complicated the operation, and rendered its after treatment more difficult. The linseed poultice alone seems to me, therefore, a safer remedy.

Now let us turn to the other form of disease. Where a false membrane is really present the whole category of symptoms is very different from those last described. This illness is not

dangerous only from its position; it is not a mere catarrhal state of a mucous membrane, but it is a disease in itself, and the production of the false membrane is merely a phase in it. The patient, if he be old enough to complain at all, speaks of feeling ill, and of some soreness about the throat; but although there has been premonitory fever, there has not been, as in the last disease, premonitory cough. It is not, however, impossible that the croupal breathing or croupal cough (for cough is then present) may be the first stage in the disease for which the mother requests the advice of her doctor, but the history will show that these have not been the first symptoms. On examining the throat, the uvula or tonsils are generally found more or less coated with the well-known diphtheritic membrane which has caused the "croup" by extending into the larynx. The history of this case shows that the child has been depressed and feverish for a time varying from a few hours to as many days, and that this has been followed at first by symptoms of sore throat rather than of cough. The last was a chest disease, proceeding upwards to the larynx; this is, as far as its external signs show, a throat disease passing downwards, and the difference in symptoms is to be looked for accordingly. All know that an elongated uvula does give cough; that a swollen epiglottis does the same; that the swelling of larynx would equally produce it, and that even before the invasion of a diphtheritic membrane; so that there may be a certain kind of cough history; but that has not been the special symptom—not, as in the cases of laryngitis before mentioned, the only symptom in a child otherwise well. Sometimes, however, the little patient is not seen until urgent laryngeal symptoms are well set in, and then, if there be no history, and also, from the urgency of suffocation, much difficulty in examining the throat, the diagnosis is one of real difficulty; but it is not then of vital importance, since in such a case the immediate performance of tracheotomy is certainly necessary. It is my decided opinion that sometimes cases of diphtheria occur in which no false membrane is visible from the mouth; yet it must be confessed that I have never made any notes with a view to the investigation of this special point, and there is some difficulty in getting a thorough look at the throat of a living child when suffering from laryngeal dyspnoea. Moreover, post-mortem examinations are apt to slur over the mouth, uvula, tonsils, and pharynx; yet I have notes of several cases where no throat diphtherite is mentioned, and where my firm impression is that none was present; and there certainly is no known reason why such should always be the case. There would, of course, be rather more difficulty in distinguishing these from catarrhal croup, yet this is in most cases rather an imaginary than a real difficulty. Albuminuria



is a frequent symptom in diphtheria, but by no means constant enough to form a ground for diagnosis. If present in a doubtful case it might certainly clear it up; but, on the contrary, its absence would prove nothing. It is, therefore, the early symptoms, and the presence or absence of exudation in the throat, on which we mainly rely. If the child has had shivering and fever with sore throat before the laryngeal symptoms began, if there is a history even of general malaise before the croup commenced, we have a right to expect the presence of exudation, and when present it will most frequently be easily seen.

The next question which arises is, Do the two diseases require any difference in treatment so as to make a clear diagnosis of importance? Such is most decidedly the case. Catarrhal laryngitis is dangerous only from its position, and we have to subdue it by prompt and active measures; but in diphtheria we have a depressing blood-poison, dangerous in itself, quite independently of its position, and our lowering treatment is useless, or worse—positively injurious. Emetics are even to my mind doubtful. Cases are on record, certainly, where tubes of false membrane have been said to be brought up by their action, yet these must have been in a very different state from that in which we generally see them, as they do not usually adhere so loosely as to be got rid of in this manner, and we have no right to expect any such result. Still, one or two full emetics may be tried; but, these failing, do not steam the child as in laryngitis. Do not add to the depression by mercury, but give him some supporting mixture, such as the *tinctura ferri muriatis* with *liquor ammoniæ acetatis*; and if laryngeal symptoms have set in and there is real dyspnoea, if the breathing be laboured and the sternum drawn in with every breath, do not wait for symptoms of impending suffocation, but operate at once—the earlier the better. The false membrane in the larynx will probably spread further downwards; moreover, the blood-poisoning continues, and to wait for blue lips means to wait for pneumonia also. When the larynx or trachea is thoroughly invaded you cannot operate too early; delay means death. It must be remembered that the operation does not check the disease; but as we have no specific treatment that can stop it, perhaps simple support after the immediate risk of suffocation is over is as good as any other. —*Medical Times and Gazette*, August 31, 1872, p. 221.

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## 27.—CHLORAL IN CROUP.

By Dr. JOHN BARCLAY, Banff.

My first case was that of a child of fifteen months, who had first tonsillitis, then hoarseness, and croup supervening. During the three days after the larynx became involved the case went on like an unfavourable case of croup, from bad to worse, and at the end of that time little or no hope was entertained of recovery. Tracheotomy was proposed twice; the first time it was postponed because, just when the operation was about to be performed, some improvement took place; and then at a later period it was again proposed, but rejected as holding out next to no hope of saving the child. The distress of breathing was extreme, and more to relieve this than with any hope of ultimate benefit, I ordered two grains of chloral, with two minims of ipecacuanha wine every two, three, or four hours, according to the effect produced. Ipecacuanha wine alone had been given before. Besides these sherry was given freely. I watched the case carefully and closely all along, and these were the observations I made. Before the chloral was given the respirations were from 60 to 70 per minute, and the pulse varying from 144 to 156, and weak; the dyspnoea was extreme, and the voice and cough now close and dry. Within two hours after the first dose great relief in the breathing was observed, the child slept for half-an-hour at a time, the respirations and the pulse fell a little, and, what was more curious, a very copious exudation of mucus began in the larynx and trachea. Indeed, so great did this become that it was necessary to raise the child every half-hour, when a little water or sherry was given to produce a cough, by which the air-passages were cleared of the mucus. Had the child been allowed to lie it would soon have been suffocated, for after the chloral was given and the exudation had commenced, there was not the slightest inclination to cough. Gradually the pulse fell, and the respiration became slower, till at the end of twenty-four hours the former was only 108, and the latter 30. This treatment was persevered in for forty-eight hours, after which milk and sherry only were given. The recovery was slow but steady.

The next case was likewise one of inflammatory croup in a child of four years. There had been hoarseness and croupy cough for two days before I saw him. I ordered chloral in four-grain doses, with four minims of ipecacuanha wine, every four or six hours, and nothing else in the way of medicine. In two days the hoarseness and croupy cough had disappeared, the symptoms of improvement being the same as in the former case—namely, quiet sleep, exudation of mucus, and decrease of fever.



During the past two years I have treated several other severe cases of croup with the chloral and ipecacuanha, and I have not been disappointed with the result. In all of them I have made it my first object to obtain a quiet sleep, and on the child awaking I have almost always found some decided improvement.—*Lancet*, September 21, 1872, p. 404.

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## 28.—ON THE TREATMENT OF HOOPING COUGH.

By Dr. WILLIAM MACCALL, Manchester.

In the management of a simple case of pertussis our first endeavours must be directed to the prevention of complications. The child should be sufficiently clad, and as far as possible guarded against exposure to variations of temperature, especially sudden variations. The food should be given in smaller quantities, and more frequently than usual; and should be of such a nature as (varying according to age) is most easily assimilable by the child. Where vomiting occurs so frequently as to interfere with nutrition, I am in the habit of ordering a small quantity of some light farinaceous food, or of beef-tea, to be given just after a paroxysm, as it is then most likely to be retained long enough on the stomach to allow of its being digested wholly or partly before vomiting again occurs. Complications occurring must be treated on general principles, with especial care to avoid depressing remedies, because we may still have the pertussis to deal with after the intercurrent malady has been subdued.

But after adopting these general precautions must we be content to remain expectant? In the mildest cases we *may* do so; but where the paroxysms are severe, and threaten to induce nervous or other dangers, we cannot simply stand by looking on; and fortunately we are not without remedies which have a marked influence in lessening the intensity and duration of the disease. In an affection which is so variable in these respects, statistics showing the length of time it lasted under different forms of treatment might be in the highest degree deceptive. We must be guided in our judgment, as to the comparative value of various medicines, by carefully watching the features of each case, and the apparent control which a remedy exercises over it, occasionally intermitting it as a further test. By pursuing this method in a large number of cases we can arrive at definite and useful conclusions, which would nevertheless lose rather than gain in clearness by being grouped statistically. I will therefore state very briefly the result of experience gained in this way.

Foremost among the remedies of this class I should decidedly place opium and belladonna. The former I have generally used

in the form of Dover's powder, either alone or combined with camphor, about gr. j. thrice daily to a child of a year old. Its effect in lessening the number and intensity of the attacks is in many cases most marked, as well as in soothing the general irritability which often exists. Its tendency to produce constipation is sometimes objectionable, and where there is much moisture in the chest it is better not to use it.

Belladonna is the drug which seems to exercise the most constant and potent control over the paroxysms. I have generally used the powdered leaf, following the usage of the hospital—sometimes the solution of sulphate of atropia. As the tolerance of belladonna varies extremely, it is well to begin with small doses (*e.g.* gr.  $\frac{1}{4}$  of the powdered leaf two or three times daily for a child a year old), and cautiously increase till we arrive at a dose which exercises an influence over the paroxysms. To effect this it is not necessary to produce decided atropism, and where this does occur it is well to reduce the dose or stop the remedy for a short time. I have found it equally effective, and more convenient in dispensary practice to give small repeated doses instead of the single dose once a day, or once in two days, recommended by Trousseau. As I have not pushed the remedy to heroic doses, I have not seen any alarming results from its use. In many cases, instead of constipating, it has a tendency to act gently on the bowels.

*Bromide of Potassium* was often used in these cases; but I have found it to be a much more uncertain remedy. In many cases its beneficial action is very marked, while in others it seems to be entirely ineffectual. It is specially valuable where a convulsive tendency exists. It has the advantage of not requiring much caution in administering; but sometimes it is not well borne by the stomach. To a child a year old about gr. ij. may be given three times daily, or oftener.

I have lately used *chloral*, and so far my experience of it has been favourable. It ought to be given in pretty full doses, (say a grain for each year of the child's age) and repeated every four hours. In severe cases it is often advisable to give it in such doses as will induce sleep each time. I have found it answer very well in some cases where convulsions occur.

Of some of the other remedies, such as hydrocyanic acid, sulphate of zinc, chloroform, topical applications, &c., I have not sufficient experience to speak definitely.

In cases where there is abundant secretion of mucus, especially in the later stages, I have found dilute nitric acid of much value. It seems to lessen the secretion and exercise an antispasmodic and tonic action. In continuing its use we must not forget its effects on the children's teeth. In cases where there is much prostration carbonate of ammonia is nivaluable.



Emetics are useful adjuncts at all stages of the complaint, and in a few cases I have treated it by them alone. I have most frequently used powdered ipecacuan; in cases where there is much depression sulphate of copper is preferable.

Where the disease is protracted, and accompanied or followed by debility and emaciation, cod-liver oil (pure or etherised) is the most potent means we have for restoring health and preventing sequelæ, especially of a tubercular nature.—*Glasgow Medical Journal*, Aug., 1872, p. 510.

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### 29.—CONSUMPTION AND CONTAGION.

By Dr. RICHARD PAYNE COTTON, Senior Physician to the Hospital for Consumption, Brompton.

Numerous as have been the believers in the contagiousness of phthisis, no one had fully developed and openly advocated such a doctrine until Dr. William Budd published his startling, but clearly expressed and evidently well matured views upon the subject. In this paper Dr. Budd expressed his conviction that consumption is a zymotic disease of specific and highly contagious nature, closely allied in such respect to the most contagious diseases with which we are acquainted—viz., scarlatina, typhus, and syphilis, the tubercular material itself being the specific and morbid poison in which the malady originates, and by which it is disseminated through society.

Notwithstanding the high authority under which this view of phthisis is propounded, I most unhesitatingly declare my conviction of its fallacy. Consumption I believe to be a purely constitutional disease, which may be either inherited or acquired, but which, from its very nature, is incapable of being conveyed from one person to another in the ordinary sense of the word contagion. I have, indeed, never met with a case of pulmonary phthisis, however apparently due to a contagious origin, which could not be much more easily and rationally accounted for by generally recognised causes, than by the doctrine of specific and contagious influences. It is quite true that persons previously in fair health are sometimes seen to succumb to phthisis after having attended the last illness of some consumptive relative or friend; but when it is remembered that anything which lowers the physical and nervous powers and depresses the general health is of itself sufficient either to originate phthisis or to develope some hitherto latent tendency to it, we have, I consider, a sufficient explanation of very many such cases, and might even wonder that they are not more frequently happening. In all instances of phthisis originating under such circumstances, it has long been my habit to make, as far as

possible, a complete inquiry into their general, and more especially their family, histories; and I have invariably been led to the conclusion that the long nursing and the close connexion with the disease was at most the *exciting* cause of its subsequent development, the predisposing one having been, for the most part, sufficiently apparent, either in a previously existing hereditary taint, or in some other cause far removed from the idea of contagion. If consumption were "caught" like a zymotic disease, how few in comparison must be the instances of escape to those of infection after contact with it; whilst it must be in the experience of us all that cases, arising even under the most suspicious circumstances as regards contagion, are by no means common, and bear a very small proportion to those in which, after the closest contact with the malady, no ill effects have resulted.

It must nevertheless be admitted that cases now and then occur in which consumption has become developed in the widowers or widows of those who have died of the disease, and under circumstances apparently favouring the doctrine of contagion. I have met with several such instances; but from notes taken at the time, I find the very curious fact that they have chiefly occurred amongst women. Such cases, however, do not, in my opinion, give the least support to the doctrine of contagion, but may be explained in a very different way. The union of husband and wife is not only a religious and social, but also a *physiological* one, as may be easily shown by our daily experience. It has often been remarked, for example, that after a variable period of married intercourse, the one person is seen to resemble the other not merely in manner and habit, but in feature and expression. The offspring, too, of second marriages has been frequently known to resemble both in feature and character the husband of the previous union. So well established, indeed, is this corresponding physiological fact with respect to the lower animals, that it has to be ever borne in mind in their breeding and rearing. If, therefore, certain natural and healthy peculiarities can be imparted from husband to wife, or *vice versâ*, there is every reason for concluding that morbid diseases, peculiarities, and tendencies may equally be imparted. But there is another and a less mystical way in which it may fairly be concluded that phthisis may be conveyed from husband to wife. A phthisical father may communicate to the *fœtus in utero* a consumptive taint, which may thus be indirectly transmitted to the mother, just as we well know is often the case with secondary syphilis. And it may be due to this circumstance that, as I have already observed, phthisis arising under such apparently contagious conditions is much more frequently met with in women than in men. I believe, there-



fore, that in one or other of these ways—either by a transmission through the offspring, or by a mysterious and inexplicable connexion between husband and wife—phthisis may sometimes be developed in either of them; and we ~~that~~ need not resort to zymotic influences in explanation of such an occurrence.

Upon the question of contagion, however, practical evidence is of far more value than anything speculative; and as a great experiment upon the zymotic character of phthisis has long been going on at the Consumption Hospital, I shall briefly give the results.

The Consumption Hospital was opened in the year 1846, with ninety beds. Ten years later, it was completed; and since that time, two hundred beds have been constantly occupied. We have lost, during this long period, only one nurse from phthisis; and this was a poor creature whose husband had deserted her, and who had long endured, from other causes also, considerable mental anxiety and physical exhaustion. On the other hand, the services of the nurses generally have been unusually prolonged; and I can myself testify to their general health being, as a rule, remarkably good. Of those *now resident*, two have been at duty in the hospital seventeen years; one has resided thirteen years; one, eleven years; two, ten years; two, nine years; one, seven years; one, four years; two, three years; and four, two years. The two oldest nurses have lately died of old age and general decay, after having long been superannuated; each of these had resided in the hospital for upwards of twenty years.

The present chaplain has been with us for twenty-two years; and his two predecessors are still living. Happily all of them are well.

Of the staff of physicians and assistant-physicians—nineteen in number—one only has been affected with phthisis; and he was a young man of delicate and decidedly consumptive aspect. The period of office which some of us have had is unusually long. Of the present physicians, two have been upon the staff twenty-four years each; one for seventeen years; one for thirteen years; one for nine years; and one for five years. The late senior physician, who resigned only three years ago, had been attached to the hospital, and in constant work there, from its very foundation. Two of the former physicians who resigned their appointments, and subsequently died of disease quite distinct from phthisis, had held office in the hospital for fifteen and nineteen years respectively.

With the above facts before us, must it not appear to all believers in the doctrine of phthisis originating in a special and contagious poison, that a residence in the Consumption Hospital, and long-continued working in its wards, is a very good

way indeed *not to catch the disease?*—*British Medical Journal*, Aug. 31, 1872, p. 239.

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### 30.—A REMEDY FOR CATARRH.

Dr. E. BRAND (*Berlin Klin. Wochenschrift*) speaks in terms of recommendation of the following formula for an antieatarrhal olfactory, prescribed by Dr. Hagner: R Carbolic acid, 5 parts; rectified spirit of wine, 15 parts; strong solution of ammonia, 5 parts; distilled water, 10 parts. The mixture is kept in a stoppered dark glass bottle. When a catarrh is commencing, a few drops are placed on three or four layers of blotting or filtering paper; the patient, holding this in his hand, and closing his eyes, inhales deeply from it as long as any smell is perceptible. The effect of the treatment is to cut short the acute stage of the cold, to prevent the occurrence of subsequent coryza and bronchial and laryngeal catarrh; while all troublesome symptoms are rendered much milder. The remedy should be applied every two hours.—*British Medical Journal*, July 6, 1872, p. 12.

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### DISEASES OF THE URINARY ORGANS.

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### 31.—IMPROVED METHOD FOR THE MICROSCOPIC EXAMINATION OF URINE, &c.

Whatever can diminish his labour and save his time must be welcome to the busy practitioner. The ordinary method of examining urinary deposits microscopically entails a considerable expenditure of both; and the process has generally to be repeated several times in order to discover all the characters of the deposit. By the use of the simple contrivance of a “submersion tube”, first described by Dr. Dudgeon, in Vol. xi. of the *Quarterly Journal of Microscopical Science*, this labour is greatly diminished, and a large quantity of the urine can at the same time be examined at one operation. The submersion tube is simply a brass tube closed at the end by a thin plate of glass, which is screwed on to the objective, so that the latter may be dipped into the fluid under examination, which is contained in a glass tank or trough placed upon the stage of the microscope. The urinary deposit is, in most cases, quickly thrown down upon the bottom of the trough, and thus the examination of a large quantity of urinary deposit is at one time made quite practicable. The advantage of such a plan as this will be apparent in many cases; as, for example, in the urine of patients suffering from contracted granular kidney, with few renal casts.



The only points to be attended to in the construction of the submersion tube are, that the length the tube projects beyond the object-glass shall be less than the focal distance of the latter, and that the thin glass plate shall be cemented to the brass tube in a perfectly water-tight manner. As the fluid in the trough must be kept horizontal, the microscope of the ordinary construction must, of course, be used perpendicularly, so that if we wish to be seated while making our examination, the microscope should commonly stand on a low table, or a common wooden chair. Objectives of various powers, fitted with a submersion tube, are very useful for examining minute aquatic, vegetable, and animal organisms in a considerable quantity of fluid. They are especially adapted for watching the development of the ova of fishes, amphibia, and molluscs, for examining the circulation in the transparent membranes of fishes, and for all other purposes when it is necessary that the object under examination should be immersed in a considerable quantity of fluid. The examination of vomited matters, neglected as a rule by practitioners, will be, in some instances, also greatly facilitated by Dr. Dudgeon's submersion tube.

Mr. Adie, of Pall Mall, or any other optician, can make a submersion tube to fit on to an objective of any power up to a quarter of an inch, and perhaps even to objectives of higher powers, though, for ordinary purposes, it is not necessary to go beyond the quarter-inch objective. It is scarcely necessary to remove the submersion tube when examining objects in the ordinary way between two plates of glass, for the thin glass plate that closes the end of the tube does not appreciably affect the distinctness of definition of the object seen through it. The glass trough should be made of pieces of plate glass cemented together with marine glue. For examining urine it need not be more than two inches square and one inch deep. We are of opinion, from a careful trial with the submersion tube, that its advantages are such as to encourage and simplify the use of the microscope in the wards and in private practice.—*British Medical Journal*, March 23, 1872, p. 314.

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32.—CASES ILLUSTRATING THE ACTION OF CONTINUOUS ELECTRIC CURRENTS ON SPASMS OF THE BLADDER, URETHRA, AND URETERS.

By Dr. RELIQUET, of Paris.

I.—In the month of March, 1869, I had a patient suffering from stone in the bladder, with contraction of the bladder over the stone. Micturition took place almost every instant, and it was quite impossible to inject more than ten grammes of tepid water into the bladder.

I resolved to make use of electricity, and, with my friend Dr. Onimus, applied continuous electric currents in the following way. A gum-elastic sound, furnished with a very flexible stilet, was introduced into the bladder; the usual ten grammes of water were injected, and the sound closed. The positive electrode was then applied to the stilet, and the negative (a large moist plate) to the hypogastrium. At the beginning of the current there was a little pain, which soon disappeared, and then complete composure ensued. After four minutes the currents were stopped; then tepid water was injected into the bladder, and as much as 150 grammes were introduced without the patient feeling the desire to make water. I availed myself of the opportunity to examine the stone, which I found lodged in the vesical fundus, where it was impossible to lay hold of it with the lithotrite. The painful spasms having returned under the provocation of these exploring manœuvres, I resumed the continuous currents, which immediately brought back the calm which they had at first produced.

This brusque dilatation of the bladder occasioned the unloosening from the vesical parietes of crystalline incrustations, which the patient passed during the forty-eight hours which followed this first application of electricity.

On the following days I renewed the application of electricity, and each time I invariably obtained the same temporary sedation of the vesical contraction. However, after the first micturition which followed, the spasmodic condition of the bladder again returned.

In this case the problem so eagerly sought to be obtained by the most various means, and especially by the local action of carbonic acid—namely, *to dilate the contracted bladder*—seems to have been solved.

Consecutive applications of continuous electric currents in various cases of painful spasms of the urinary passages, have determined in a definitive manner the practical importance of this novel remedy.

II.—In the month of October, 1871, I again applied continuous electric currents to act against the contraction of the bladder over a calculus. The results which I obtained were absolutely identical with those of my first case. Only thirty grammes of tepid water could be tolerated by the bladder: by means of the current I was enabled to inject 130 grammes without bringing on any desire to pass water. But, as in the former instance, the desire to pass urine became more and more frequent after the first micturitions which followed, and the condition of contraction did not fail to return: the direct cause of the phenomenon, namely the stone in the bladder, being still present.



III.—In the month of January, 1870, I attended a young man of twenty-one, who came to consult me, he said, for catarrh of the bladder. And, indeed, the urine contained much mucus. Moreover, the desire to pass water was most frequent. The jerking jet of water at the commencement of micturition did not persist; at the end the urine passed in drops. There was constant pain in the anus, and pricking sensations in the neighbourhood of the glans. Pain was especially intense after each micturition. On examining the bladder, I found that it did not empty itself at each micturition, though it could not bear more than 100 grammes of tepid water. Yet I could find no calculus. After this investigation, which had been carried on with the slightly curved instruments, the patient was two days without feeling any pain in the anus or penis. They again appeared, however, after that time, and renewed depression of the lower lip of the neck of the bladder produced no effect.

I then applied the continuous electric currents. As in this case it was contraction of the urethra which dominated, the bladder not emptying itself entirely, I applied the negative electrode in the bladder, and the positive electrode over the hypogastrum. After having thus made several intermittances, I allowed the continuous current to go on. Then, putting the positive electrode on the perinæum, I obtained a current crossing the deep region of the urethra, and going from the perinæum to the bladder. After five such applications the desire to make water was only felt three times in twelve hours; the bladder could receive 220 grammes of tepid water without there being any wish to urinate, and furthermore could effectually empty itself. Pain had completely disappeared; motions, which had been difficult, had now become easy, natural, and daily. The urine contained scarcely any mucus.

Walking, and especially dampness of weather, however, had the effect of awakening the condition of pain. But new applications of the continuous electric currents arrested the painful spasms, and again caused disorders of micturition to disappear.

At the end of February, the patient, on returning one night from the theatre, was seized with a violent nephritic colic of the right side. At the same time the urine was very thick, and micturition became quite frequent. I immediately had recourse to the continuous currents, applying the positive electrode to the back in the situation of the right kidney, and the negative electrode over the perinæum. I made a long *séance* of a quarter of an hour. After this the night was passed in complete calm, and the patient passed in making water, almost without being aware of it, a tolerably large quantity of gravel. On the following days I continued the employment of the electric currents, still following the direction from the left kidney to the bladder.

Complete recovery became more and more evident. Ten days after the escape of the gravel, I carefully examined the bladder with the exploring lithotrite, but discovered nothing. The bladder could then tolerate 220 grammes of tepid water, and micturition took place regularly every three or four hours. The urine was quite normal.

It seems obvious from this fact that (1) continuous electric currents first cause the arrest of spasmodic phenomena of the urethra and bladder excited by renal gravel and by the alteration of urine which results therefrom; and (2), a most important fact, that the action of electricity has been most effective against the spasms of the right ureter, since, by occasioning the cessation of the spasms, it excited the evacuation of the renal gravel.

In the following case, which I have briefly summed up, it will be seen that, in a very irritable subject, electricity, in causing the disappearance of the spasms of the urethra, also caused the permanent disappearance of disorders of micturition.

IV.—In the month of August, 1871, Mr. H., aged forty-five, of an extensively developed neuropathic temperament, came to consult me. He had just returned from Contrexeville, whither he had resorted with the hope of getting rid of various urinary troubles from which he had been suffering for several months.

On examining him I remarked the presence of the following symptoms:—Frequent desire to urinate: nine or ten micturitions in the night; not quite so many during the day. There is a thick sediment of muco-pus in the water, but no crystals or earthy matter. Moreover, the urine has no smell of ammonia. Each micturition is accompanied with an intense burning sensation in the urethra, and followed by a painful sensation going from the anus to the meatus urinarius. There is habitual constipation.

This constipation I immediately attacked by prescribing a glassful of Pullna water every two or three days, and the use of fresh vegetables.

On August 29th, I introduced easily into the urethra a gum-elastic sound of five millimetres in diameter. Its passage through the deep region of the urethra excited intense pain. By means of the sound I drew from the bladder more than 200 grammes of urine, a much greater quantity than that afforded by each micturition, and which proved that the bladder did not entirely empty itself at each micturition. After having injected tepid water into the bladder, I attempted to introduce the exploring curved catheter; but as soon as the beak of the instrument attained the membranous portion, intense pain ensued, and consequently contraction of the urethra, which completely put a stop to any further introduction. I therefore contented myself with passing a metallic sound, with a less



marked curve, by the aid of which I explore bladders in which I have discovered nothing.

After this exploration the pain in making water became much more intense, and micturition still more frequent.

The following day, August 30th, I applied continuous currents, putting the positive electrode in the bladder, by means of a curved sound furnished with a brass wire (moving freely within the sound), and the negative electrode over the perinæum.

After five minutes' employment of a perfectly continuous current, I applied the negative electrode to the hypogastrium, where I made various interruptions of currents. In this way I made four *séances* of electricity, one every day; and then four others, one every two days; making in all eight sittings.

From the very first sitting, the pain in making water became much less intense. As pain diminished, so the quantity of urine afforded by each micturition became greater, and the desire to make water more and more rare. After the sixth *séance* there were only two micturitions in the night, and during the day the patient remained more than three hours without any wish to urinate.

The water gradually became normal as the disorders of micturition disappeared. I may just state that the patient was taking an infusion of pine-buds.

Two months later, at the end of October, I saw Mr. H., and he was still doing remarkably well.

As is shown by this case, the spasms of the urethra cease and the complete contractility of the bladder disappears, under the influence of continuous electric currents properly applied.

In cases of spasms of the deeper portion of the urethra due to chronic urethritis of this part of the canal, I have always obtained the abatement of the spasms, and consequently a diminution of functional troubles. The patients make water less often; the water does not run out by jerks, and the comparatively considerable effort made to eject the urine, which is so slow in issuing, decreases much, and often is reduced to the normal effort which attends the beginning of micturition. But in these cases, when we do not succeed in dispelling suppuration of this deep portion of the urethra, all the spasmodic phenomena return as soon as the applications of electricity are suspended.

This abatement of functional troubles procured by the continuous currents is a most favourable condition, which increases the efficacy and play of the therapeutical means employed in chronic urethritis.

When, whatever may be the cause of functional troubles of micturition, the patient is an irritable subject, there is need at each *séance* to begin by electrising the lower part of the spinal

cord with a descending current: the reflex power is thus diminished. It is by this means that Dr. Onimus has been enabled to cure nocturnal incontinence of water in several children; but with these patients it is important not to act directly upon the genital parts.

We are now in possession of a means of action against painful spasmodic phenomena of the urethra, of the bladder, and of the ureters, and, what is very important, of a remedy which produces no inconvenience in the general organism, as have done all the medicinal substances employed until now, as, for instance, opium, belladonna, and bromide of potassium. Moreover, as all the facts of my experience have shown me, the action of continuous currents is immediate. We are able to calm instantaneously pain produced by the contraction of the bladder over a calculus, and that until the bladder again becomes irritated over the stone. And, during this time of temporary dilatation of the bladder, of vesical tolerance, we are enabled to act on the stone. Whence arises a most important indication amidst many others, namely, the possible advisability of applying continuous electric currents immediately before a *séance* of lithotrity.—*Practitioner*, Aug., 1872, p. 98.

### 33.—ON A METHOD OF DETECTING SMALL QUANTITIES OF SUGAR IN URINE.

By Dr. J. SEEGEN, Professor in the University of Vienna.

Trommer's is the most reliable and delicate test for sugar. With its aid, I am able with certainty to make out 0·3 *milligramme* (0·0046 grain) of sugar dissolved in 10,000 times the amount of fluid. This great delicacy of the test, however, only holds good as long as we have to do with a watery solution of sugar. If, on the contrary, small quantities of sugar are to be detected in urine, Trommer's test is neither delicate enough nor reliable, for two reasons. 1. Urine contains certain substances (colouring matters, creatine) which prevent the suboxide of copper when formed from being precipitated; no separation of the reduced suboxide of copper, therefore, takes place, the blue fluid only becoming yellow or yellowish-brown or presenting a turbid discoloration. 2. The same processes of reduction are also brought about by uric acid; and urine, containing a considerable amount of uric acid, acts on Fehling's test-fluid exactly in the same manner as urine containing 0·1 to 0·2 per cent. of sugar.

The method devised by me has for its object the exclusion of those other constituents of urine which would disturb the proper action of the test, and the transformation, as it were,



of the saccharine urine into a watery solution of sugar. Animal charcoal has the property of retaining most of the constituents of urine, more especially the colouring matters and uric acid. After filtering a watery solution of uric acid through animal charcoal I could (provided the charcoal had been good), after repeated filtrations, not find a trace of uric acid in the filtered fluid. Now, in order to detect small quantities of sugar in urine, I proceed in the following manner.

I filter one or two ounces of the urine several times through good animal charcoal until the urine is completely colourless. This operation only takes a few minutes. Then I wash the charcoal on the filter with a little distilled water, and to this water, when filtered off, I apply Trommer's test. The water with which the charcoal has been washed is almost as sensitive to Trommer's test as a watery solution of sugar, and in it I could detect even 0·01 per cent. of sugar by a beautiful red precipitate of suboxide of copper, whilst the original saccharine urine, when not filtered, only produces a yellow discoloration of Fehling's test-fluid. With urine containing a little more sugar—say 0·1 to 0·2 per cent.—the water flowing off from the second and third washing acts even more energetically upon the test-fluid than that of the first washing, producing an even purer deposit of suboxide of copper. The water obtained by the subsequent washings thus evidently contains the sugar in a purer form. With normal urine, the water obtained by the above process is either entirely inactive towards Fehling's test-fluid, which remains blue, or it assumes only after a while a slight dichroid (varying colour according as the light falls on or passes through) turbidity. The water obtained by a second or third washing always remains without any effect. When the quantity of sugar has to be determined, the urine must not be filtered through charcoal, as the latter always retains a certain quantity of the sugar which cannot be removed again by washing.—*British Medical Journal*, May 4, 1872, p. 469.





# SURGERY.

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FRACTURES, DISLOCATIONS, AND DISEASES OF BONES,  
JOINTS, &c.

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## 34.—PLASTIC APPARATUS IN SURGERY, WITH ESPECIAL REFERENCE TO THAT VARIETY MADE WITH PLASTER OF PARIS.

By Dr. SAMUEL B. ST. JOHN, New York.

I boldly assert my belief that no other apparatus is so successful for treatment of fractures generally as the so-called immovable apparatus, and no other variety of that apparatus so good as that made with plaster of Paris or gypsum. This assertion compromises me to the utmost, for it is the expression of a conviction founded upon extensive observation and personal experience, and sustained by theories, some of which are original, so far as my knowledge goes.

Regarding materials, we may say that leather, felt, paste-board, and gutta percha—all of which may be softened in water—have always borne a high reputation. Liquid applications, to be applied with a brush or the hand upon a substratum of cloth, such as solutions of starch, glue, dextrine, or water-glass, have given a very brilliant series of results, and under this head may be classed that variety of the plaster of Paris splint made by soaking thick cloths in a plaster “cream” (made by mixing gypsum with water to the consistency of ordinary cream). The latest modification of this type of apparatus is made by using the dry plaster incorporated with the meshes of an ordinary bandage, as will be described subsequently.

With regard to the method of application of plastic apparatus generally, we may remark that the earliest were made in a very clumsy manner by placing the limb in a box and pouring the plastic material around it, or by smearing it over the outside of the limb, as was done in those alluded to as being made of clay or mortar. The first advance was in wrapping the limb in some fabric, such as cloth or matting, which was smeared upon the outside. These methods secured a strong splint, it is true, if made thick enough, but exceedingly heavy and difficult to remove. A great improvement was made by soaking the

cloths in the plastic material, made temporarily liquid by some menstruum—usually water. It was then found that equally strong splints could thus be made, which were much thinner and lighter than before, and the advantage thus gained by a combination of the fibre of the cloth with the resisting material led to a more intimate and uniform combination by making the splint out of roller bandage, saturated with the liquid starch, dextrine, or gypsum. An improvement, in my opinion, though not an essential modification is the use of the roller bandage having the plastic material rubbed in dry, as will be explained hereafter.

The plaster bandage of Mathiesen has lately received a thorough trial at Bellevue Hospital, and with the most gratifying results. It combines the firmness and solidity of the plaster with the tenacity of the cotton fibre, thus securing a firm and tough splint with a minimum of plaster, and, by consequence, a minimum of weight, with the additional advantage of application in a form which enables the surgeon to secure the most accurate adaptation to the limb under treatment. The plaster bandage is prepared by rubbing the fine, dry plaster into the meshes of a cotton or flannel bandage, the coarser the texture the better (within limits), as it holds more plaster. I have used also bandages of canton-flannel, and coarse linen (towel-lin). The linen ones made the toughest and lightest splint, and ordinary flannel next best. Lately tulle has been used, and it is highly spoken of as making a lighter splint than any other material, but it is too weak to be snugly applied. Tarletan was the material used extensively by the Germans in the late Franco-German war, and they strengthened their splints by their wooden ribbons, pieces of which were worked in longitudinally. A fine film of plaster is then dusted over it and the bandage rolled up rather loosely. These bandages may be kept ready for use in tin cans with tightly-fitting covers, or each bandage may be separately rolled up in tin-foil. In making a splint for a fractured leg, for example, the leg is first enveloped in a layer of cotton wadding, or, what I prefer, a single thickness of soft blanket. If cotton is used it is best to hold it in place by a roller bandage, loosely applied over it. Burggraave preferred to use a layer of cotton one and a half inches thick, and no dry bandage, which he claimed was so elastic that it followed the diminishing leg as swelling subsided. The blanket will need no bandage, but may be secured by a few stitches at intervals. The plaster bandages are then immersed in a basin of hot water to which about 3ij of common salt have been added (the high temperature and the salt both contributing to the rapid setting of the plaster). In about three minutes they will be saturated, and are ready to be applied. Extension then



being made upon the foot, and the bones adjusted, the plaster bandage is taken from the basin, squeezed firmly to get rid of the superfluous water, and applied after the fashion of an ordinary roller, from the base of the toes up to or beyond the knee, each successive turn being drawn snugly to its place, but not too tightly, especially if the bandages have never been washed, as they will in that event shrink a little in drying. Experiments show that the plaster in setting does not contract at all, so that there is no danger of ligation from that source. Three, or at most four, bandages should make a firm splint from the toes to the knee. In applying the roller, should the plaster, which it holds, seem to dry, the hand may be dipped in the basin and rubbed over the dry surface, and indeed, it contributes to the firmness and elegance of the splint to keep the hand constantly passing over the successive turns of the bandage, rubbing in the plaster and smoothing the surface. An elegant finish and, at the same time, some additional strength may be given by mixing 3ij of the plaster with so much water and smearing it smoothly over the surface. It is best not to apply a dry roller outside, until the splint hardens enough to hold the parts in position, as it retards the setting by preventing evaporation, but the limb should be carefully held by the surgeon for a few minutes, and then placed upon a rubber blanket until it becomes quite dry and hard, when a dry bandage may be applied, if desired, though it is not at all necessary.

Thus far, the application in an average case of simple fracture has been considered. Several details, which will at once enter the mind of the surgeon, must now be considered. First. How soon may the splint be applied? Let us see what was the practice of our predecessors. Larrey, as a rule, applied it immediately, except where there was any considerable swelling, or where the muscular contractions were increased by the unavoidable handling of the limb. Velpeau says "apply at once in all cases where there is not swelling, or where the soft parts are healthy, for swelling after fracture comes too soon for inflammation, and is mainly from extravasated blood, and an early reduction and complete retention of the fragments in place will prevent irritation, the initial step of inflammation, and prevent additional rapture of bloodvessels." Scutin (*op. cit.*) applied it at once, believing that it acted to prevent inflammation in still another way, the uniform compression of the bandage (according to his view) restricting the arterial, while it favoured the venous circulation. In a late article Volkmann advocates immediate application, arguing that it tends to restrain traumatic reaction. For my own part, I believe that an intelligent surgeon, who can see his patient every day, and who employs the precautions alluded to, need not hesitate to apply the splint as

soon as the patient comes into his hands. I speak advisedly in this, having repeatedly applied it while the patient was suffering from the shock of the accident, in injuries ranging from simple fracture of the fibula up to compound fracture of the femur, and in no case have I had occasion to regret doing so, but in all the cases the fixation of the irritating fragments has seemed to act as a prophylactic against swelling and general nervous irritation. In average simple fractures "constitutional symptoms" were almost unknown. These statements are supported by the cases given tabularly, where in eighty cases of simple fracture this splint was applied within twenty-four hours from the time of the injury, in many of them within two or three hours, and the fact that fourteen of these were from direct violence shows that cases were not selected which were less apt to be complicated by swelling. In twenty-six other cases the splint was put on within forty-eight hours, three being the result of direct violence.

Secondly. In what cases should cotton wadding be used as a lining, and in what cases blanket? A year ago I employed cotton where, from the contusion of the soft parts, great swelling might be anticipated; since I regarded it as a more elastic medium. At present I use blanket in every case, because it is more evenly distributed over the surface of the limb, and thus by giving more even support tends to prevent swelling, and also because by making an early section of the plaster casing it can be loosened a little should necessity arise.

Perhaps this reference to section of the apparatus will afford the most natural approach to an explanation of the manner in which it may be done, without hurting the patient or injuring the splint. This section may be most easily made about two hours after the application of the splint, as it then cuts like hard cheese. If, however, the section is delayed until the splint has become dry and hard, it may be softened in the track of the proposed section, by boiling water applied with a sponge for a few minutes. (Nitric acid is used by some for this same purpose, but it ruins the knife, sometimes goes through and blisters the skin, and does not make the section any easier.) A common shoemaker's knife, *kept sharp*, is the best instrument to use in opening the splint, and the risk of cutting the patient may be reduced to a minimum by making two sections an inch apart, one on either side of the median line nearly through the plaster, and then raising the intervening piece at the top and completing the section by alternate nicks on either side. By thus removing a strip one inch wide a firm hold may be secured of the cut edge of the splint so as to spring it open to inspect the limb, and it also affords an opportunity of tightening the apparatus, should it have become loose by subsidence of swelling.



Should the swelling, however, have gone down so much as that it is necessary to remove a strip more than an inch wide in order to make it hug the limb, it will be better to remove the splints and make a new one, otherwise the tendency will be to roll the bones in toward the median line of the limb, in the case of the leg or forearm especially. I have tried various kinds of cutting pliers, Henry's, Seutin's, &c., and saws, also, but found none so serviceable as the knife. An instrument made by Leiter, of Vienna, a gentleman thoroughly versed in the management of gypsum apparatus assures me leaves nothing to be desired in this direction. Its mechanism is such that a tremendous leverage is obtained, so that when the lower blade is slipped between the splint and skin, the upper one cuts down upon it through the splint. The instrument has just been introduced into this country, and promises well, but I desire nothing better than a sharp knife.

In compound fractures the method of its application varies. If the opening through the soft parts is not large, and there is not much contusion or laceration, an apparatus like that for simple fracture may be applied, and a fenestra cut, through which to dress the wound. It must be made heavier and stronger than if the fracture were simple, but as the patient is to remain in bed with the limb supported for several days, if not weeks, this is not a disadvantage. In several such cases I found that five bandages were usually needed, and the splint was, in all cases of fracture of the leg, extended above the knee. The fenestra should be cut in accordance with the principles given in describing the longitudinal section of a splint for simple fracture. Some make a practice of leaving the piece covering the wound attached by one side like a trap-door, but in my opinion, it is neater to remove it entirely, as it enables the surgeon to trim the edges of the trap all around in a uniform manner, and moreover, the trap-door is apt to be in the way when the wound is being dressed. This method of procedure is, however, applicable only when the trap is to be of moderate size, of a breadth, perhaps, one-fourth of the circumference of the limb. The *length* of the trap is nearly immaterial, as it does not materially diminish the supporting power of the splint. When, from any cause, such as the extent of the external wound, or the suspicious look of the soft parts (and this applies equally to simple fractures with great contusion) it becomes necessary to have a large surface accessible to inspection without disturbing the fragments—a combination of the plaster bandage with blanket soaked in gypsum “cream” is to be used—and it was this combination under the name of the “interrupted splint” which, during the past year, secured such brilliant and gratifying results at Bellevue Hospital in the treatment of bad compound fractures.

To apply this, the limb must be thoroughly inspected to find out what parts are least contused and least liable to become the seat of local trouble. It is only necessary to find a strip of sound tissue about two inches wide connecting the limb below the fracture with that above. If that can be found on the anterior surface of the leg or arm, so much the better, as the connecting band between the two parts of the splint will then be out of danger of being soiled by discharges, or of being wet when the wound is syringed out. A piece of ordinary blanket is then to be cut, about eight inches wide, and long enough to extend well above and below the joints on either side of the fracture. (In fracture of the leg it is usual to have this reach from the toes to the middle of the thigh.) The limb having been previously encased in blanket, as described for simple fracture, a plaster "cream" should be made, and the blanket previously cut out, immersed therein—or, which is preferable, the blanket may be laid upon a table and the mixture thoroughly rubbed in with the hand. It should then be folded upon itself twice so as to make a strip two inches wide, and laid along the part of the limb previously determined on. A plaster bandage should then be applied in the ordinary way below the point of fracture, binding the limb to the connecting-piece and extending to as near the point of fracture as it is deemed safe to allow the limb to be concealed from view. Up to this point the limb has been supported by the hands of assistants, but no attempt made to reduce the fracture thoroughly. Now, the lower half of the apparatus being completed, an assistant, grasping it, makes strong extension (if necessary) and the bones are accurately adjusted. The upper end of the connecting band is then to be fastened to the limb above the fracture by turns of a plaster bandage. Thus an apparatus is made, fitting the limb accurately above and below the seat of injury, and having its two parts connected by a stiff bridge two inches wide—or, in other words, it is a plaster splint with an immense fenestra. In such an one which I made for a hopeless case of compound fracture of the leg, where the man refused amputation at first, the leg could be exposed from about three inches above the ankle-joint to two inches below the knee-joint, and the apposition of the bones which could be distinctly seen at the bottom of the wound, was perfectly maintained until the amputation two weeks later. In this case, owing to the oblique course of the crushing force (car-wheel), the only sound skin lay in a spiral line which I followed with the connecting band. If the connecting band is of necessity placed upon the depending side, it is of advantage to keep it away from the limb just opposite the wound by means of a thin blanket-pad placed under it; this pad to be removed after the



splint has hardened, leaving a space through which a strip of oiled silk may be passed for the protection of the splint—the remainder of the space to be stuffed with cotton or other material easily changed when soiled. Indeed the trap should be edged all round with oiled silk tucked under the edges, and cotton should be placed between the oiled silk and the skin to prevent the discharges from passing under the splint.

Another way which is very elegant, and which is of use in cases where the wound is not very large, is to cut, in a large piece of oiled silk, a hole a little larger than the wound, and then to fasten the edges of the hole to the skin around the wound by means of collodion. This collodion, being insoluble in the discharges, keeps the oiled silk so closely applied that the discharges are forced to find their way out through the hole in the silk, where they are received into a compress of oakum bound on. It will sometimes be found useful to paint the splint with a solution of gum shellac in alcohol ( $\frac{3}{4}$  j to Oj) which gives it a water-proof finish. This, however, has two disadvantages; it prevents the transudation of perspiration, and it renders it somewhat difficult to enlarge the trap, should that be necessary, since it gives the surface a glazing which is extremely hard in itself and prevents the use of water to soften the splint in the track of the proposed section. In case the connecting band is found to be too weak, it may be at any time strengthened by additional strips of soaked blanket laid along it and secured above and below by turns of plaster bandage. In some cases where the connecting bridge was of necessity so narrow that I feared it would be too weak, I inclosed a stout piece of hoop-iron between the folds of the soaked blanket, the iron reaching about three inches above and below the proposed trap, the ends being imbedded in the more solid parts of the splint. This is also a useful plan to adopt when there is reason to fear that the trap might have to be enlarged and more strain brought to bear, consequently, upon the connecting band.

*Special Fractures.—Fracture of the Femur.*—Perhaps in no other fractures have the merits of the gypsum apparatus been so conspicuous as in those of the femur, “it having triumphed here, not only by the brilliancy of the results, but by the comfort afforded the patient during convalescence.” A simple fracture of the femur treated with any other form of apparatus compels confinement to bed for six weeks on the average, with the consequent lowering of vital status, discomfort to the patient, and trouble to friends and attendants; while with this the patient may be out of bed upon crutches the second or third day after the fracture, and is as comfortable and nearly as well able to wait upon himself during his whole convalescence, as is

a man with simple fracture of the tibia in the fourth week of treatment under the old regime. And the results as regards shortening, time and strength of union, and subsequent usefulness of the limb, surpass statistically those of any other method of treatment, as a glance at the cases given further on will show. In the words of a surgeon whose name is identified with a different plan of treatment, "the statistics thus far are better than those of any other method," and the refusal of a prominent teacher to recommend this method, is founded, as he himself acknowledged to me, on the belief that special experience is required to apply it properly rather than in any distrust of the method itself.

A description of the method of application is essential to the completeness of this paper, and I give it rather with that view than with the design of improving upon the admirable paper of my friend Dr. Bryant.

A table must be obtained and an upright bar about one inch in diameter secured to the middle of one end, so as to rise about two feet above the top of the table. In Bellevue Hospital an iron bar is kept which fastens on by clamps, but in private practice I have used with entire satisfaction, a wooden bar one and a half inch in diameter, put through a hole bored in the end of a common kitchen table and lashed to the braces below. This upright should be thinly padded and covered evenly with a roller bandage. If the padding is too thick it will be in the way; if too thin the bar may excoriate or cause undue pressure. The patient is to be placed upon the table with the nates well down towards the edge, and the upright projecting between the thighs, so that the perineum rests lightly against it. My practice has been to place another table in line with the first and abutting against it, to support the legs of the patient and to afford a point from which to make extension. The provision for extension may be made in one of two ways, by traction upon a so-called Buck's extension apparatus, of adhesive plaster, which should be made with a very strong but narrow loop from malleolus to malleolus, so as to be in the way as little as possible, or, which is preferable, by a clove hitch around a stout plaster splint put on the day previous from the toes to the knee. A blanket previously spread upon the table should then be cut and drawn around the injured thigh so as to form a neat covering, and a strip eight inches wide should be extended around the pelvis. This should be drawn smooth and stitched as in a fracture of the leg. A strip of blanket one foot wide and four feet long (the relay), folded upon itself twice, so as to make a strip three inches wide, should be placed with its centre resting on the perineum and the ends brought obliquely upward and outward, the anterior



passing over the crest of the ilium and the posterior over and above the tuberosity of the ischium. While the blanket is being adjusted, the patient may be etherized, and, as soon as fully relaxed, extension should be made, the scrotum (in the case of a man) having been carefully drawn upward and toward the sound side. The upright bar must be made to bear a little to one side of the median line to avoid injury to the urethra, and especial care must be taken that the blanket strip, called the "relay," should rest symmetrically in the flexure of the thigh and perineum, so as to present a groove along its centre in which the upper edge of the inner side of the splint may rest. Extension is to be made by compound pulleys until the leg is by actual measurement as long as its fellow. If the lower half of the splint has been put on previously, as mentioned above, a trap may be cut over the malleolus to obtain a point from which to measure. Before applying extension strongly, however, the pelvis should be raised about four inches from the table by means of a stout sling, passing around the upper part of the pelvis and over a stout wooden bar stretching from the top of the upright to a stool, about two and a half feet high, placed upon the table above the patient's head. Pillows should be placed under the head and shoulders to elevate them to a line with the pelvis. The bandages are now to be applied, piecing out the part already on and completing the splint as you pass upward. Pains must be taken to make a neat spica and not to allow the "relay" to become displaced. Small pieces of blanket soaked in plaster "cream" may often be used with advantage to strengthen the anterior and antero-lateral portions of the upper part. Felt or pasteboard may also be used for this purpose, being worked in between the layers. About ten bandages will be needed in all, including those used in making the lower half. After the application is complete, the patient should be lowered to the table, but the extension should be kept up until the splint is quite hard. The projecting ends of blanket should then be cut off and the perineal edge trimmed with oiled silk to prevent injury by wetting, especially in children.

In Vienna, the upright used for counter-extension has a projection from its side, about six inches from the top of the table, on which the tip of the sacrum is made to rest, the body and upper lumbar region being supported by a frame about eight inches high which rests upon the table. In one of the cases given in the table, Dr. Bryant used no upright for counter-extension, but made traction instead upon the "relay" in the perineum. This idea has been still further practicalized by Dr. Van Wagenen, at present connected with Bellevue Hospital, by passing counter-extending bands over the perineum and

having brought them up one on each side, fastening them to a frame upon which the patient lies, which frame raises his body a few inches from the table, while his pelvis projects beyond its edge so that the hands of the surgeon can readily pass between that and the table.

In its application to *fractures of the tibia and fibula*, there is nothing especial to be said except that, these being the most common fractures, this method has here received the most thorough trial. Three good bandages (five yards long) will make a stout splint for most adult legs, unless the fracture should be near the knee, when four may be required, as it is then essential to carry the splint half way up the thigh, and it should in all cases extend to the toes when first applied to insure quiet of the leg muscles. In cases of high fracture of the tibia, it is often advantageous to make a circular amputation of the splint above the ankle after two weeks and begin passive motion of the ankle-joint. This is preferable to removing the leg temporarily from the splint. When there is great tendency to overriding, a good plan is to flex the leg upon the thigh sufficiently to enable the posterior surface of the thigh to act as a surface of counter-extension, the splint being carried above the knee. In Pott's fracture I have used with satisfaction, to produce inversion of the sole of the foot, a Dupuytren's splint applied outside the splint to give it shape while hardening, obviating the necessity of holding it, which may be tedious if the plaster is poor.

One case of Pott's fracture under my charge, complicated with backward dislocation of the foot, was so admirably maintained in position by a gypsum splint as to elicit the commendation of an avowed opponent of the system, who said he could not think of any other apparatus which would fulfil all the indications. When it is desirable to have the lightest possible splint, paste-board, gutta-percha, or felt may be worked in between the successive layers of bandage, and they will be held so tightly as to be in no danger of slipping even if the splint be entirely removed and replaced. Two strips of felt and two plaster bandages thus disposed make an elegant, effective, and very light apparatus. The use of alum water (*živ to Oij*) in which to soak the bandages renders it possible to make a stiff splint out of two good plaster bandages, but in my hands they have proved brittle, not having elasticity enough to allow of their being sprung off (should there be occasion) without damaging them. Such splints, however, answer admirably after being cut down not only in front, but also posteriorly, leaving only two points of attachment of the two halves, one at the heel, the other at the top, so that the two halves were merely hinged together. The edges of these splints for the leg may be bound with adhesive plaster, so as to prevent their being broken by the



surgeon's fingers in springing them open to inspect the leg, and this contributes much to the elegance of their appearance.

In a few cases of *fracture of the patella*, the plaster bandage has been used with remarkable success; in no case, so far as my knowledge goes, with a decidedly bad result. When the joint is tense with effusion, it cannot well be applied, but if there be only a moderate amount it may be used, and indeed, has been, with such a close union of the fragments that it was pronounced bony union. In these fractures, as there is usually little or no contusion, a roller bandage may be substituted for the blanket lining. Two firm compresses having been laid, one below the lower fragment, the other above the upper one, and secured by strips of adhesive plaster, the plaster bandage is then applied from the ankle up, making a figure of 8 around the knee and continuing the splint well up toward the groin. This should be applied while the patient sits on the edge of a table or chair to relax the extensors. Some place behind the popliteal space a light wooden splint, about four inches wide and a foot long, so that more tension may be used in drawing the fragments together by the figure of 8 turns without danger of constricting the limb. It will be seen that the summit of the knee may thus be left exposed for examination of the fracture, and it seemed, in two cases falling under my notice, that there was a decided tilting forwards of the fractured ends from the strong backward pressure of the bandage upon the other extremities of the fragments. This, it was claimed, could be counteracted by carrying additional turns of the bandage directly across the line of fracture, but it has the disadvantage of hiding the fracture completely from view. The number of cases is not, however, yet sufficiently large to enable one to cover up such a fracture with confident expectation of a good result. A compromise may be made by treating the fracture through a large fenestra cut in a stout plaster splint reaching from ankle to groin, which forms a posterior splint incapable of displacement, securing rest of the quadriceps extensor muscle, and affording very convenient points of attachment for straps, strings, or adhesive strips, or whatever one may be using for the approximation of the fragments. In that plan of treatment, which consists in drawing the fragments together by strips of adhesive plaster crossing each other, this form of splint is especially useful, since traction can be made directly downward and the strips fastened to the front of the splint by a few turns of a dry roller, instead of passing downwards and *backwards* as in the ordinary posterior splints, and tilting the fragments forward as already alluded to. If, as some hold, the separation of the fragments is due mainly to effusion into the joint, the success already attained by the use of the bandage alone may be due to pressure stimulating absorption.

In simple *fracture of the ulna* it has in several instances proved itself to be all that could be desired. But as this is not a severe test of its powers it may be passed by with the remark that those who believe in interosseous pads for fracture of the forearm can mould a plaster splint before it hardens by indenting it firmly along the interosseous space, so as to make a prominent ridge when viewed from the inside. For convenience in allowing a coat sleeve to be worn, it is certainly admirable, while impatient but imprudent patients, who would rebel against wearing clumsy wooden splints, by way of precaution, after a fracture had united with tolerable firmness, will readily submit to a light and comfortable gypsum one.

In *Colles's fracture of the radius*, its advantages over other methods are not so evident. Still its record here is by no means a bad one. I succeeded in two cases in obtaining excellent results by using the gypsum bandage from the elbow to the wrist-joint, making the splint especially strong and thick at its lower edge, and holding the fracture reduced by pressure upon the splint until it was quite hard. In one case I used small pads upon the radial and ulnar borders of the lower part of the forearm, so that the splint could be more firmly applied without risk of arterial and venous obstruction. Of course the fingers and hand had to be bandaged with dry rollers. In fractures of the radius high up, the recorded cases are all in its favour. In fracture of both bones with tendency to shorten, counter-extension should be taken from the anterior surface of the upper arm, the forearm being flexed at a right angle and the splint extending about three inches above the elbow-joint.

In *fractures of the humerus* low down the forearm should be flexed at a right angle, and the splint applied from the wrist to the axilla. If there is much shortening, two methods are in use, one that just given, relying upon the weight of the splint for extension; the other where the splint is extended over the shoulder, forming a shoulder-cap, by means of the spica, which closely hugs the axilla and takes its counter-extension from that part. It is claimed by some, that any effective counter-extension from the axilla would check circulation through the axillary vessels, but evidences of serious obstruction complicated none of the cases in which I applied it although the upper edge of the splint was well imbedded in the axilla. The counter-extension was derived, it seems to me, mainly from the angle of the axillary border of the scapula with the long head of the triceps muscle, the scapula being tolerably well fixed by the spica and a body bandage.—*American Journal of the Medical Sciences*, July, 1872, p. 75.

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## 35. — HIP AND SHOULDER DISLOCATIONS, AND THEIR REDUCTION WITHOUT TRACTION.

By Dr. RUSHTON PARKER, Demonstrator of Physiology and Histology at the Liverpool School of Medicine.

[The reader may with advantage refer to Mr. Hutchinson's article on this subject in our last vol., p. 172.]

In the communication of my friend, Mr. Henry Arnott, published in the Journal of 24th February, 1872, he quotes some sentences from Mr. Hutchinson's abridged comment on the work of Dr. Bigelow, and further states the undoubted fact that in some London medical schools, University College and Guy's being named, it is taught that in the easiest method by which dorsal dislocations of the hip can be reduced, flexion of the hip is required. Now, Mr. Hutchinson's remarks, as quoted by Mr. Arnott, do not necessarily ascribe to Dr. Bigelow any *priority* in his advocacy of the flexion method of reduction; still less does Dr. Bigelow claim it for himself.

Mr. Hutchinson expresses the opinion, and I think with reason, that "the researches of Dr. Bigelow in respect to our subject" are "amongst the most valuable of recent additions to the science of surgery". Mr. Hutchinson's further assertion that "Amongst the special services which Dr. Bigelow has done, is that of explaining the peculiarity of 'the dislocations into the ischiatic notch,' and of teaching us how to reduce them" is perfectly true, for Dr. Bigelow tells us far more about this form of dislocation than any one else does, and he shows why it is not, and cannot be, a dislocation into the sciatic notch.

Dr. Bigelow fully acknowledges the work of other men on the same subject, and on pages 27—30, he makes a brief historical allusion to the flexion method; citing, among others, Nathan Smith as far back as 1831, but distinctly mentioning the practice and advocacy of this method by Hippocrates. In France, M. Dolbeau is said to have advocated flexion in 1867, attributing its introduction to M. Desprès, in 1835. It is well-known that cases of hip-dislocation reduced, some by flexion and rotation, others by flexion and traction, have from time to time been reported by British and French surgeons in recent years; but Dr. Bigelow has undoubtedly demonstrated how *all* recent dislocations of the hip may be reduced by flexion and other manœuvres without traction.

I do not purpose going more fully into Dr. Bigelow's work, which has less than 150 pages of large print, and is really a charming book, fully warranting encomiums even higher than those of Mr. Hutchinson; but I will now state the practical grounds upon which I ask its due for a set of principles which

constitute a true "theory of hip-dislocations and of their replacement."

1. I have, on the dead body, repeatedly practised that part of the demonstration of this subject which can be done when the soft structures around the hip-joint have been removed. With the femur connected to the pelvis by only the Y-ligament and the internal obturator tendon, all the four classical dislocations were made—the position of the limb in every case being exactly that described and figured in text-books as occurring in the living body. The reduction of these (and of others) was in every case done by flexion and rotation, with adduction and abduction as required; the course taken by the head of the femur, and the part played by the Y-ligament, being *seen*.

2. One afternoon in July or August, 1869, there was brought to University College Hospital a little girl eight or nine years old, who said that, while riding behind a carriage in the Hampstead Road, she had had a fall which immediately resulted in the injury to be described. I saw her about half-an-hour after the accident, and found the right thigh fixed in a state of extreme flexion, the knee being bent and the leg rotated out. A dislocation of the hip was vaguely suspected, but the child would not permit the least tactile examination, any movement of the affected hip causing her to scream out. Chloroform was given to full anæsthesia, during the production of which Dr. Poore, then resident medical officer, brought a dried pelvis to assist the diagnosis. The head of the femur, on careful examination and slight rotation, could now be felt near the tuber ischii. Our concurrent diagnosis was "dislocation on to, or near, the tuber ischii." When the child was fully under chloroform, by suggestion a few circumductions of the thigh were gone through—in the order of flexion, abduction, and extension. As this did not remove the deformity, I did what I had at first intended to do. The ankle and knee were grasped, the knee and hip being flexed; and the femur was rotated in; the head of the bone immediately slipped backwards an inch or two (as I thought, towards the sciatic notch). At once the femur was rotated out, and then the head slipped into the acetabulum, not with a loud snap, which I have never heard in reducing dislocations, but with a sort of "crunch." The actual process of reduction took about five seconds. A starched bandage was applied, and the child was well in two or three weeks, all deformity being absent. At this time I knew nothing of Dr. Bigelow's classical work, nor of the part played by the Y-ligament; or I might have caused the head of the femur to enter the acetabulum by the first manœuvre. I now believe the dislocation to have been "vertically downward."

3. Cases have been from time to time published during recent



years, of hip-dislocations reduced by flexion with traction, and by flexion without traction but with rotation and other manœuvres. To the former class belong Mr. Quain's case quoted by Mr. Arnott; a case of Mr. Hulke's; and that of Mr. J. W. Macdonald, and others. To the latter belong the following:—a case of Dr. Hector C. Cameron (*Glasgow Medical Journal*, July, 1867), who reduced a so-called sciatic dislocation in a boy of 14, by flexion and rotation, traction having failed; a case of Mr. Annandale (*British Medical Journal*, 29 January, 1870), who reduced by flexion and rotation, and afterwards dissected, a thyroid dislocation in a man aged 19; a case of Mr. Lister's, the reference to which is not at present within my reach, but which was, as far as I remember, one of the downward dislocations, closely resembling my own case, and reduced by the same method.

I would add that the Y-ligament includes the ilio-femoral and ilio-trochanteric ligaments described in Ellis's *Anatomy*, which are one at their upper attachment, rather below the anterior inferior iliac spine.

Dr. Bigelow, then, fully recognising the efforts of others in this direction, has elaborated the great principle which places this department of surgery upon a scientific basis. His book should be read by every practitioner; after which, the physical error and barbarous act of approaching a recent dislocation of the hip with pulleys should be for ever scouted.

There are still barbarities practised on the shoulder-joint when it is "out." I know that cases have been published in which luxations of the humerus have been replaced by manœuvring; further, at University College, Mr. Marshall advises it in his lectures on the subject, and describes the process. But the method is not yet largely practised, as far as I know, by house-surgeons, and they are the men who probably come across most of the cases; besides, the ordinary methods generally succeed so well, that it has not become urgent to use this. I did not attempt it properly in a single case when I was a house-surgeon. The following are my grounds for advocating the primary adoption of this method (described with the cases) in shoulder-dislocations, recent and moderately old.

1. I saw a case at University College Hospital in which one of the house-surgeons failed, with chloroform and the heel in the axilla, to reduce a forward and inward shoulder-dislocation, though much force was used. He then elevated the elbow to a level with the shoulder, rotated the arm, and at once reduced the dislocation.

2. My friend and fellow house-surgeon, Mr. R. T. Smith, tells me that he had nine cases, in none of which he used chloroform, and in eight of which he reduced the luxation by

this method with little or no pain to the patient, and in an exceedingly short space of time. In one case this method was not tried.

3. I have since had a case myself in which this method was practised. A man, aged 31, fell in getting off the top of an omnibus in Liverpool on October 19th, 1871, dislocating the head of his right humerus inwards and forwards. My father and I were with him soon after the accident, when he was rather "screwed," and his muscles were relaxed. After stripping his shoulders, I raised the elbow on the dislocated side till it was level with the shoulder, then pulled back the elbow in the horizontal plane, by which the head of the humerus was brought outwards by leverage—the glenoid fossa of the scapula, I imagine, being a support for the neck of the humerus to rest on as a fulcrum. Rotation was followed in a few seconds, with but little effort and no pain, by reduction. I believe, but am not quite sure, that rotation *in* was the last reducing manœuvre.

This is the method which I should employ again in subcoracoid and subclavicular luxations. For downward dislocation, probably great elevation of the elbow would be enough; and for the backward dislocation, elevation of the elbow to the level of the shoulder, leverage of the head of the humerus by drawing forwards the elbow, and, if necessary, rotation. Concerning the two latter luxations, I only say what seems to me the requisite action; but I have never tried it.

This communication contains nothing new that I am aware of, but it deals with matter of fact throughout, except in the speculations about shoulder-dislocations.—*British Medical Journal*, Sept. 7, 1872, p. 271.

### 36.—ON THE TREATMENT OF "PERSISTENT" INFLAMMATION BY THE LOCAL APPLICATION OF SOLUTIONS OF MERCURY AND MORPHIA.

By JOHN MARSHALL, Esq., F.R.S., Professor of Surgery in University College, London.

[The solutions used by Mr. Marshall consist of oxide of mercury dissolved in excess of oleic acid. A small amount of morphia is then added to the solution, and this also doubtless combines as a base with oleic acid. The salts of morphia are not soluble in oleic acid. By "persistent" inflammation is meant inflammation which has become prolonged or persistent in a given locality—whether chronic, subacute, or acute.]

This prolongation or "persistence" of local inflammation may be due to various causes, local or general. Thus the parts



affected may not be kept sufficiently at rest, as when a joint is exercised too soon after injury or inflammation. Or the interruption to the cure may be due to the continued performance of some natural function—as, for example, in the case of the tonsils, the glottis, certain orifices of the body, the mammary gland, or uterus,—the condition induced, in such cases, being not so much chronic as “persistent” and severe. So, also, if the general health be disordered—if a gouty, rheumatic, or syphilitic, an anæmic or plethoric condition should exist,—a local inflammation may not disappear, but may be prolonged or become “persistent.” Again, supposing this condition to be once established, the products of the inflammatory process, either by their presence or as a consequence of the chemical changes which they themselves undergo, may excite or exhaust the functional activity of the neighbouring nerves, bloodvessels, and other tissues, and may thus induce further irritation, leading, if not to the extension, at least to the prolongation of the inflammatory process. Inflammation of this obstinate type may, of course, terminate in suppuration, ulceration, or gangrene; or it may, by some interference with the nutrition of the affected tissues, impair or destroy the elasticity or strength of parts which serve mechanical uses in the body, or deeply influence their special vital functions.

Now, in the treatment of all inflammation, three chief ends must be kept in view—namely, the relief of the local nervous irritation, the stimulation of the vasi-motor nerves and the bloodvessels, and the promotion of the absorption of the inflammatory educts and products. The mode by which this absorptive process is actually accomplished is so far normal that these inflammatory products and educts undergo a series of changes—namely, softening, degeneration, and solution—quite similar to those through which certain healthy tissues pass when they are no longer needed in the economy; as, for instance, the tissues of the uterus and mammary gland after parturition and lactation are completed. In the quasi-normal as in the normal process, the veins and lymphatics are the agents of absorption. Finally, when the morbid constituents or products are removed, the nervous irritation and the vascular disturbance are allayed, and the part regains its ordinary or healthy condition. In many cases, time, rest from mechanical use or functional activity, and freedom from disturbing influences, are sufficient to ensure this natural process of recovery. In a certain number of instances it will happen that a purgative, a diaphoretic, a stimulant, a tonic, or a sedative, will indirectly promote absorption; and thus salines, alcohol, quinine, iron and morphia, may really act as absorbent remedies, and so especially will food and pure air.

But in the management of the prolonged or persistent inflammation above referred to, further local measures are often indispensable; and, for the purposes of practical illustration, let us suppose we have to deal with persistent inflammation of a *joint*. Here, as of primary importance in the local treatment, must be mentioned immobility and rest, as secured by means of proper splints or other apparatus, accompanied by the use of a sling for the upper limb, and by the maintenance of the recumbent posture for the lower limb. Next may be considered the application of uniform and judicious pressure, by means of cotton-wool confined by suitable bandages. Heat and cold have also their peculiar advantages in particular cases. Lastly, certain medicated applications have frequently to be resorted to. Whether these are employed in the form of lotions, fomentations, liniments, ointments, or plasters, they usually contain one or more of the following substances as their active ingredients—viz., alcohol, ammonia, camphor, turpentine, cantharides, iodine, or mercury, with or without chloroform, opium, morphia, or belladonna. Each of these most efficacious remedies is, no doubt, adapted to particular stages of persistent articular inflammation; but every surgeon meets with numerous instances of such inflamed joints which, in spite of care and good treatment, continue obstinately uncured for weeks or months. With some, blisters, and with others, iodine, is the favourite remedy in such cases. Scott's ointment and the mercurial liniment are likewise often employed with great advantage, in both of which applications mercury is the most active ingredient. But mercurial ointment, the basis of these preparations, is merely a mechanical mixture of minute globules of mercury, or of solid particles of the black oxide, with some unctuous substance; and I have long thought that, if we could employ a *solution of mercury in some oleaginous or unctuous medium*, we should obtain more immediate, rapid, and satisfactory results from the well-known therapeutical powers of this ancient remedy. The other mercurial ointments now in use, made with the nitric oxide, the subchloride, the ammoniochloride, or the red iodide, are also essentially mechanical mixtures, and all, as well as the nitrate ointment, are too irritating to be used as topical absorbents. In seeking for my object I first dissolved some of the perchloride of mercury in a small quantity of ether, and added to it about four times the amount of oleic acid; but I found that this combination freely used on the skin produced much irritation, unless it was employed in too dilute a form to be of service as an absorbent. In Gmelin's chemistry there is a short account of certain metallic oleates formed by double decomposition; but with this as a guide, I failed to obtain any satisfactory oleate of mercury. My friend,



Mr. Frank Clowes, to whom I then referred the chemical question, soon discovered that, although the ordinary sublimed scales of red oxide of mercury were with difficulty dissolved in oleic acid, the oxide, precipitated by caustic potash or soda from a solution of the metal in nitric acid (which is a yellow impalpable powder) is, when recently made and well dried, readily soluble in oleic acid, especially when aided by a temperature of about 300° Fahr. At my request Messrs. Hopkin and Williams have since studied the subject pharmaceutically, and have succeeded in preparing oleate of mercury, and certain solutions of that salt in oleic acid. The strength of the preparations made by them is indicated by the per-centage of the oxide of mercury which they contain. The 5 per cent. solution is a perfectly clear pale-yellow liquid, resembling olive oil, but thinner; the 10 per cent. solution is also fluid and perfectly clear, but as dark as linseed oil; whilst the 20 per cent. preparation is an opaque yellowish unctuous substance, closely resembling in appearance resin ointment, melting very readily at the temperature of the body, and forming a kind of transparent, viscid, colourless varnish when applied to the skin. The chief care to be observed in the manufacture of these solutions is not to hurry the process, and not to employ a high temperature, or the mercury will be immediately reduced.

Unlike the mercurial ointment so long in vogue, which is a crude, gross, unscientific mixture, very dirty and very wasteful, because so small a proportion of its mechanically admixed mercury is but slowly absorbed, these solutions of oleate of mercury are cleanly and economical in use; and as the diffusibility or penetrating power of oleic acid is much greater than that of ordinary oils or fats, and as each one-thousandth part of even a minim of these new preparations contains its proper modicum of mercury, they are absorbed by the skin with remarkable facility and manifest their remedial effects with great promptitude. They should not be rubbed in like ordinary liniments or embrocations, but should be *merely applied with a brush, or be spread lightly over the part with one finger*; otherwise they may cause cutaneous irritation, or even produce a few pustules on the skin, especially in certain persons. This result may, however, be obviated by the addition of a small quantity of olive oil, or purified lard, according as an oleaginous or an unctuous preparation is required. Any of these forms may be scented by the addition of essential oils.

In employing these mercurial solutions for combating persistent inflammation of joints, I soon found that the addition of morphia was of very great advantage. For this purpose the simple alkaloid must be used, as neither the hydrochlorate, the acetate, nor the meconate is soluble in oleic acid. For every

drachm of the solution of oleate of mercury in oleic acid one grain of morphia may be added. Being, as well as the mercury, completely dissolved, it quite as rapidly penetrates the skin, comes quickly into contact with the extremities of the nerves, and thus, even within a few minutes, acts upon them at their most sensitive points, and speedily produces a soothing effect.

The oleates of mercury and morphia, thus united in one preparation, represent, as it were, a liniment, ointment, or plaster of mercury and opium; but they are far more elegant, economical, and efficacious. As a rule, according to the size of the part affected, from ten to thirty drops are sufficient for one application. This should be repeated twice daily for four or five days, then at night only for four or five other days, and afterwards every other day, until a cure is obtained. The morphia immediately begins to relieve pain, allays the nervous irritation and consequent vascular turgescence, and thus arrests the progress or "persistence" of the inflammatory process; whilst the mercury probably promotes the death and degeneration of the morbid products, and so facilitates their subsequent removal by absorption. Unless used in excessive quantity, the oleate of mercury does not salivate, or produce any marked constitutional disorder.

As some of you will remember, I recently tested, in the hospital, the value of these combined oleates in the treatment of a case of "persistent" inflammation of the knee-joint. This affection, I need hardly say, is often difficult to cure, on account of the large size of the articulating surfaces, ligaments, and synovial capsule, and also on account of the great weight supported at the joint, and the frequent disturbance of the parts in the unavoidable movements of the limb and body. Frequently, indeed, this disease leads to irreparable injury to the knee. In the case in question, occurring in a man aged forty-four, the disease had already lasted upwards of eighteen months. From effusion into and about the joint the knee was greatly swollen and disfigured; there was not only synovitis, but, in addition, inflammation and thickening of the fibrous capsule and the ligaments of the joint. He complained of great and constant pain, of a creaking feeling or noise when the knee was moved, and of occasional starting pains at night, probably indicative of threatened or actual softening of cartilage. The skin over the knee was œdematous, and deeply stained with iodine, the remedy which had chiefly been relied upon before his admission under my care. After this stain had been removed, from twenty to thirty drops of the solution of oleate of mercury and morphia (5 per cent. of the former with one grain of the latter to the drachm) were applied to the knee night and morning for about ten days, and then at longer inter-



vals. A piece of linen was kept around the joint; over this was put a smooth, thick layer of cotton-wool, and then the limb was rather firmly bandaged from the foot to just above the knee. During this treatment, the patient, who was of course confined to his bed, rapidly improved in all respects; and at the expiration of four weeks he left the hospital, able to walk by the aid of lateral leather splints, and very far advanced towards a cure. From the first day of their application the effect of the remedies was decided and prompt. I could adduce many other examples equally satisfactory. I select the following;—

A gentleman, aged thirty-one, suffered, about eighteen years ago, for some weeks from an attack of fever, followed by consecutive abscesses in the left hip-joint, which led to stiffness, shortening, and wasting of the corresponding limb. On his recovery, the right leg was necessarily subject to over-use, which, together with occasional blows received at football and otherwise, brought about repeated attacks of capsule-synovitis in the right knee. These at length resulted in chronic thickening of the joint. During the last three or four years, owing chiefly, it is said, to the increasing weight of the body, renewed attacks of inflammation more or less acute led to a state of “persistent” inflammatory disease of the joint, which became much enlarged, deformed, somewhat flexed, and almost immovable. There was pain on the slightest motion, especially on twisting the leg, and such extreme tenderness that the patient shrunk from even an attempt at examination of the knee. There was scarcely any power in the limb; and there were nocturnal startings on falling asleep. Finally, the joint at one part seemed on the point of suppurating, as indicated by increased local heat, redness, and œdema, and by the occurrence of slight shiverings. In consultation with Dr. Sheldon, I ordered absolute rest in bed, the use of two long well-fitted leather splints, and the employment of the combined oleates of mercury and morphia, as above indicated. The first application afforded manifest relief to the local pain. This and the exaggerated tenderness soon subsided, absorption was established, and the size of the joint became so reduced that the splints speedily required renewal; and in four weeks there was complete freedom from pain in and about the knee, and the patient was soon able to walk on crutches with comparative ease. Thus, in a month, a joint, already very seriously implicated, and threatened with suppuration, was practically saved. I can confidently say that I know of no other treatment which would have afforded such satisfactory and speedy relief. I doubt whether Scott’s plan would have succeeded in controlling the disease present in this knee; certainly it would not have done so with such promptitude.

I will now advert as briefly as possible to the use of these same preparations of mercury and morphia in the local treatment of "persistent" inflammation affecting other parts or organs of the body. Indeed, their applicability and utility appear to me to be almost coextensive with the occurrence of such inflammations themselves, provided only that the seat of the disease be in, or sufficiently near to, the skin.

I may first mention that not only in persistent articular inflammation, but also in simple synovitis, these remedies rapidly relieve the tenderness and pain, and promote the absorption of the fluid effused into a joint. They are also of decided benefit in the rheumatic, the arthritic, and the mixed forms of joint disease; but in these they do not, of course, supersede the necessity for general treatment. In inflammation of the mammary gland occurring during or after lactation, or altogether independently of that secreting process, their efficacy is unequivocal; for I have seen, not only the induration left after previous abscesses, speedily disappear under their use, but a tendency to recurrent suppuration in the site of old abscesses, and the threatened formation of new ones, entirely controlled and arrested. I have also seen a threatened abscess in the perineum from inflammation of one of Cowper's glands, and likewise the troublesome indurations left after ordinary perineal abscess, rapidly disappear on the use of these preparations. In obstinate and painful tonsillitis, in epididymitis, in periostitis, and in inflammation with imminent or actual suppuration in or around lymphatic glands, I have similarly employed them with decided advantage. In hydrocele they have not appeared to be useful. I have used equal parts of the 20 per cent. ointment and purified lard applied outside the eyelid, with success, in hordeolum and in palpebral conjunctivitis.

In many cutaneous affections the oleate of mercury solutions, without morphia, form elegant and powerful remedies. It was in a case of obstinate sycosis menti that I first used, and with excellent results, an ethereal solution of the perchloride of mercury mixed with oleic acid; but I now much prefer, as equally efficacious and far less irritating, the five per cent. solution of oleate of mercury in oleic acid, with the addition of an eighth part of ether. This, when applied to the skin with a camel-hair pencil, is a most diffuent and penetrating remedy. It enters the hair-follicles and the sebaceous glands, penetrates the hairs themselves, and carries everywhere with it its powerful metallic constituent. Besides sycosis, it will cure chloasma and the various forms of tinea; it is useful in porrigo and in pruritus ani et pudendi; but I have not found it serviceable in non-specific psoriasis or in eczema. The solution of oleate of mercury destroys pediculi immediately; and, owing to its



singular power of permeation, simultaneously kills the ova—a result not always certain when ointments containing undissolved mercury are used.

Again, in many of those syphilitic affections for the cure of which mercury is applicable, the oleate-of-mercury preparations offer some advantages. Thus, in congenital syphilis, a piece of the 20 per cent. ointment, about the size of a pea or bean, placed in the child's axillæ, night and morning for five or six days, rapidly and easily, and without any sign of uncleanness, produces constitutional effects. Even in the adult this mode of introducing mercury into the system, either for the cure of syphilis or other disease, may often be preferable to, and less troublesome than, the bath, and it certainly gets rid of the objections to the ordinary mode of inunction. As a topical remedy for certain local manifestations of syphilis, such as the non-ulcerated form of syphiloderma, especially when these disfigure the head, face, neck, or hands, the 10 per cent. solution is a most valuable adjunct to other treatment, the spots rapidly disappearing under its use. This or the 20 per cent. preparation, diluted with equal parts of purified lard, may also be applied to non-ulcerated syphilitic indurations and condylomata, but it gives pain if applied to surfaces much excoriated or ulcerated, to moist warts, or to mucous membranes. In syphilitic iritis, and also in non-specific forms of that disease, this diluted oleate ointment smeared over, not within, the eyelids, evidently promotes the absorption of the effused lymph. Lastly, in some of the remoter kinds of syphilitic affections, which iodide of potassium will usually cure, such as very hard nodes and certain forms of syphilitic testicle, the external application of the oleate of mercury is very valuable. I have seen a case of enlarged testicle and epididymis, the syphilitic origin of which had not been suspected, and for which no mercurial course had been prescribed, but which during a period of six years had been, from time to time, relieved by enormous doses of iodide of potassium, speedily and decidedly benefited by the inunction of the 20 per cent. mercurial oleate.—*Lancet*, May 25, 1872, p. 709.

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### 37.—ON THE SPIRAL SPRING EXTENDER ; BEING A NEW AND SIMPLE APPARATUS FOR MAKING EXTENSION IN DISEASED JOINTS, &c.

By C. HOLTHOUSE, Esq., Surgeon to the Westminster Hospital.

[This apparatus is intended for the purpose of keeping up any amount of traction on a limb or joint, without necessarily confining the patient to bed, or preventing him from taking bodily

exercise. The author considers it as efficient as Sayre's splint, and much simpler and cheaper.]

My apparatus consists of a simple spiral spring, covered with leather, flannel, or linen, and having two buckles at one or both extremities. When extended, it forms a cylinder closed at the sides but open at each end; and when the coils of the spring are pressed together it approaches in form to a discoid ring or a closed concertina. It need not, however, necessarily be cylindrical, but larger at one end than the other, in conformity with the tapering shape of the limb. It can be made of any length, size, or strength; and the traction-force exerted can be regulated with the utmost exactness; while its cost need not exceed five shillings.

*Application of the instrument.*—This is extremely simple. If the hip be the joint which it is wished to extend, two square pieces of strong diachylon plaster having a piece of bootstrapping, from six to eight inches in length, fixed to the centre of the lower border of each, should be applied to the upper half of the thigh, one on each side. The plaster should be sufficiently large to surround, or nearly so, the circumference of the limb, and should be applied evenly by their whole surface; for further security they may be surrounded by a roller bandage applied in the ordinary manner. Over the limb thus prepared, the cylinder previously compressed, is slipped, the foot, of course, being extended as in pulling on a boot, and it is drawn up the limb till its larger end is supported by the perineum; this serves as its fulcrum. The tapes on the plaster being now buckled to the lower end of the cylinder, it will be obvious that any amount of traction on the hip-joint may be obtained, according as it is buckled tightly or loosely; the first will, of course, approximate the rings of the cylinder, and so increase the traction of the spring; while, in the latter, the rings being further apart, less force is necessarily brought to bear on the joint. The exact amount of traction-force may always be known by trials made with the cylinder previous to its application, and for convenience the tapes may be divided into inches. If the knee be the joint affected, two buckles must be fixed on the upper end of the cylinder as well as the lower, and the tapes which are attached to the plasters on the thigh must correspond with their upper instead of their lower borders. The plaster must, moreover, encircle the lower instead of the upper half of the thigh. On the leg the tapes must hang downwards, and the plaster be applied just below the knee. In some cases it may be advisable to substitute splints for plaster, in which case the two ends of the cylinder may be supported by buttons or hooks on the outer surface of the splints.



Although the spiral spring extender may be most frequently called into requisition in cases of joint disease, it may be employed with advantage in many fractures and dislocations, as well as in excision of joints, when it is desirable to keep the bones asunder. In amputations, especially of the thigh, it is peculiarly eligible where the flaps have been made too short, or have become so by sloughing, the pulling forward of the integuments by this instrument taking off all tension from the sutures, much in the same way as Hainsby's instrument does in cases of hare-lip.

To sum up. The chief merits of this instrument, and its superiority over the weight and pulley, rack and pinion, and other mechanical contrivances for making extension, consist in its great simplicity, cheapness, lightness, and efficiency; the patients being in many cases able to take out-door exercise, and thus avoid the injurious effects of long confinement in bed.

The above have been made for me by Messrs. Mayer and Meltzer, of Great Portland-street.—*Lancet*, June 8, 1872, p. 789.

### 38.—NEW METHOD OF PUTTING UP FRACTURED CLAVICLE.

By Dr. LEWIS A. SAYRE, Professor of Surgery in the Bellevue Hospital Medical College, New York.

The *American Practitioner* for July, 1871, contained a description of the method of dressing fracture of the clavicle, which we are assured will commend itself to our readers. It is thus described:—

“Strong and good adhesive plaster (Maw's moleskin is the best) is cut into two strips, three or four inches wide (narrower for children); one piece long enough to surround the arm and go completely round the body, the other to reach from the sound shoulder around the elbow of the fractured side and back to the place of starting. The first piece is passed around the arm just below the axillary margin, and pinned or stitched in the form of a loop sufficiently large to prevent strangulation, leaving a portion on the back of the arm uncased by the plaster. The arm is then drawn downward and *backward* until the clavicular portion of the pectoralis major muscle is put sufficiently on the stretch to overcome the sternocleido-mastoid, and thus pull the inner portion of the clavicle down to its level. The plaster is then carried smoothly and completely round the body, and pinned to itself on the back to prevent slipping. This first strip of plaster fulfils a double purpose: first, by putting the clavicular portion of the pectoralis major muscle on the stretch, it prevents the clavicle from riding upwards; and

secondly, acting as a *fulcrum* at the centre of the arm, when the elbow is pressed downward, forward and inward, it necessarily forces the other extremity of the humerus (and with it the shoulder) *upward, outward, and backward*; and it is kept in this position by the second strip of plaster, which is applied as follows: Commencing on the front of the shoulder of the sound side, drawing it smoothly and diagonally across the back to the elbow of the fractured side, where a slit is made in its middle to receive the projecting olecranon. Before applying this plaster to the elbow, an assistant should press the *elbow well forward and inward*, and retain it there, while the plaster is continued over the elbow and fore-arm (pressing the latter close to the chest, and securing the hand near the opposite nipple); crossing the shoulder at the place of beginning, it is there secured by two or three pins.

“When this has been done, the deformity will have entirely disappeared, the fractured bones will be accurately adjusted, and as long as the strips of plaster maintain their position, no amount of force can displace them.”—*Medical Press and Circular*, Sept. 4, 1872, p. 194.

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### 39.—DR. SAYRE'S SPLINT APPLIED TO FRACTURED NECK OF FEMUR.

By WILLIAM COLLES, Esq., Surgeon to Steevens' Hospital and Lecturer on Surgery in the Hospital College.

The various applications recommended at different times, and the continued alterations proposed in the form and adjustment of splints for the treatment of fracture of the femur sufficiently indicate that a proper apparatus for these injuries is still a desideratum.

This want is more apparent in the treatment of fracture of the neck of the femur than most others; here we have to deal with, in general, an infirm old female whose health is failing, who is subject to various bronchial affections, whose bones have become more liable to fracture, whose flesh is more apt to suffer from pressure or damp—all which are not found in younger persons; but besides all this, the surgeon cannot insist on the necessity of strict obedience to his directions as to the use of position and splints, as they rarely afford a prospect of obtaining a sound and useful limb.

Still we do not like to abandon these cases, and to leave them without any support or extension, or some effort to assist nature's efforts at union—bony or ligamentous.

We will not now allude to many of the various positions and appliances recommended for the treatment of this affection, as I believe the majority of surgeons use the long splint, Liston's,



or some of its various modifications; this, in a fractured neck of femur in old persons, is liable to the objections—first, that it keeps the patient's body and limbs in a perfectly straight line, preventing any motion of the patient, so that we are often obliged to leave it off or relax it, to change the position of the patient for purposes of cleanliness or comfort; or, as often happens, the breathing becomes affected, and the patient requires to be placed in a sitting or semi-erect position, and we must again relax the splint. The perineal band is also a source of suffering, causing inflammation, ulceration, or even sloughing beneath it; for it has to bear not only the pressure of the extension, but much force applied to it is lost; for if we consider the action of the long splint fixed to the body by the perineal bands, and also a girth round the pelvis, when these are tightened they act on the trochanter as a kind of fulcrum, and much force is expended in pressing this projection of bone inwards towards the pelvis, and this pressure on the fracture under consideration must add to the patient's suffering, pressing the two rough surfaces of bone together, and may interfere with the process set up for union either by ligament or bone.

I have long tried various forms and kinds of splints for the treatment of this injury without meeting any one to give satisfaction, till I read the interesting lecture of Dr. Sayre, recorded in the *British Medical Journal* (see *Retrospect*, vol. lxiv. p. 147), where he describes his splint for morbus coxæ, where the extension is fully kept up, and the patient is enabled to bear the weight of his body on the instrument with ease and comfort.

Having been called to a case of fractured neck of the femur in an old lady, I resolved to try a modification of this splint, so as to make it suitable to the injury, and quickly and cheaply constructed. The instrument consisting of three parts—first a pelvic portion about two inches deep and six inches broad, padded and falling to the brim of the pelvis lying over the trochanter and pressing on the os innominatum; this must be retained in its place by a perineal band. 2nd. The thigh splint—a bar of iron half an inch broad, joined to the pad by a ball and socket-joint—stands out horizontally for an inch or so to carry it beyond the trochanter, where it bends down and lies close to the femur, and projecting an inch or two beyond this bone. The third portion is the knee-piece—a broad band of iron embracing the front and inner portion of the knee, the remaining circle of the limb being held by a strap and buckle. This knee-piece slides along the thigh splint, and may be fixed by a screw, or, what I prefer, a strong elastic band passing from the screw head to a knob fixed on the lower end of the splint, because we have a constant and steady extension kept

up which we can increase or lessen as we find necessary. The instrument was made for me by Messrs. Adam and Corcoran, Ormond Quay. This splint is certainly open to the objection of the perineal band, but the pressure on this is reduced to the minimum, because the extending force acts on the pelvic pad, and the stronger it is the more it presses this pad against the pelvis, so that the entire strain is not on the perineal band as in the ordinary splint. In the case in which I tried it the splint gave me great satisfaction; it kept the limb well *in situ*, prevented the common shortening and eversion, and was not inconvenient to the patient; and I have no doubt but that it will be found useful not only in this fracture but I should think in many other fractures of the bone, and might even be utilized in artificial limbs, where we wish the pressure to be borne on as extensive a surface as possible—for which purpose I recommend it to the notice of the profession.—*Dublin Journal of Medical Science*, March 1872, p. 187.

#### 40.—THE ANTERIOR SUSPENDING SPLINT IN THE TREATMENT OF FRACTURES OF THE LOWER EXTREMITY.

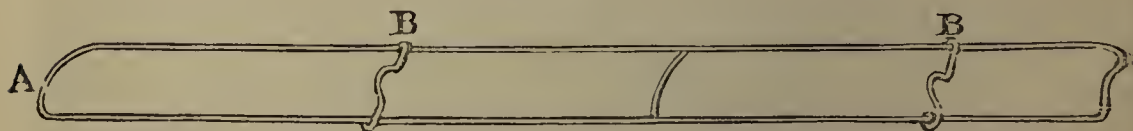
By Dr. CHARLES SHRIMPTON, Paris.

[The anterior suspending splint was invented by Professor Smith, of Maryland University, U.S.A.]

The apparatus is of itself extremely simple: a splint made of two parallel wires of sufficient length to extend from above the anterior superior spinous process of the ilium to beyond the extremity of the toes, a suspending cord, some wadding, and some bandages.

The splint I now employ is made with the parallel wires kept an inch and a half from each other by four transverse fixed bars. (Fig. 1.) B B are sliding bars, made with curves in the

FIG. 1.

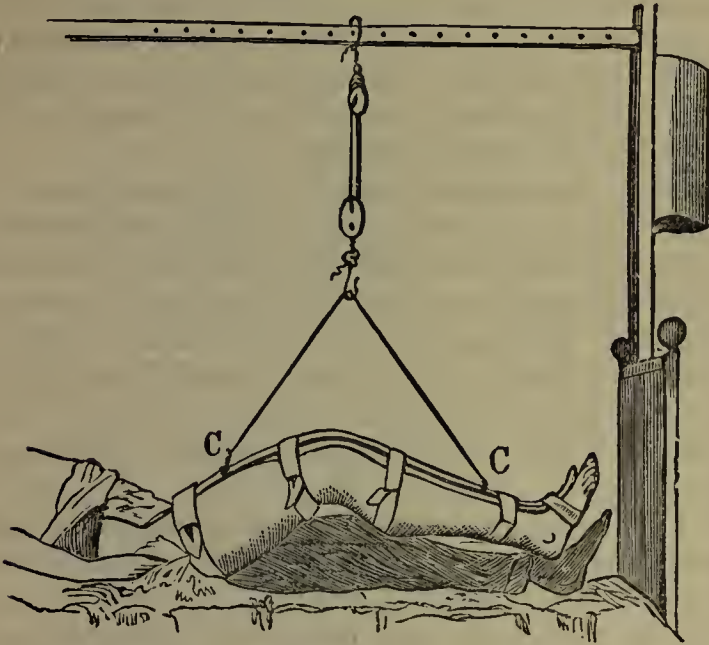


centre to receive the hooks of the suspending cord. The suspending cord is formed of two parts, one to hang perpendicularly from its support, the other with a hook at each end, to be attached to the sliding bars of the splint. This part of the cord passes through a pulley, which is hooked to the extremity of the perpendicular cord. The perpendicular cord, as seen in Fig. 2, is double, with a block in the middle, so that the limb may be raised or lowered by lengthening or shortening the cord at will.



The wadding to be placed on the surface of the limb before the splint is applied. A double stout band to pass round the pelvis, and fix the upper part of the splint. Five short pieces of band,

FIG. 2.



one to pass round the foot and the splint, two in the same way round the leg, and two round the thigh to keep the splint in place whilst it is being adjusted. Several strong rollers to be applied to the whole length of the limb, to replace the short bands, which are to be withdrawn as the roller is applied. The accessories to the apparatus are the pincers, which are very convenient for bending the splint *in situ*, to adapt it to the limb. The wood-frame supports the suspending cord. A can, trough, and syphon for irrigation, complete the apparatus.

The print represents the splint *in situ* before the roller-bands are applied. The limb is supported by the temporary short bands. The wadding has been withdrawn to show that the wires cease to touch the limb as soon as it is suspended.

Very little remains to be said in describing the mode of application of the splint. The fractured limb should be left, as it may then be, until the splint has been adapted to the *sound* limb, in order to take its exact length, bends, &c. The suspending cord now fixed, and the splint prepared with a bend corresponding to the groin of the fractured limb, we may proceed to suspend the limb. The large double thick band is to be attached to the upper part of the extremity of the splint, and passed firmly round the pelvis. The wadding is to be placed on the front part of the fractured limb to receive the splint, and the short bands are now to be tied *lightly* round the foot,

leg, and thigh. This done the limb can at once be suspended by the cord without any attempt at reduction, and the reduction will take place almost of itself by the mere weight of the limb. As soon as the splint is perfectly adjusted *parallel to the axis of the limb* the rollers may be *lightly* applied, leaving any part requiring dressing free from every kind of bandage. The wounds may thus be attended to without interfering in any way with the rest of the apparatus. Extension and counter-extension are necessary only to bring the limb to the length of the splint whilst the rollers are being applied.

The following case illustrates the advantages to be derived from the anterior suspending splint. This was a case of gunshot wound fracture of the left leg just below the insertion of the ligamentum patellæ. The subject of it was a French soldier wounded during the siege of Paris. Cure without shortening or deformity of the limb, though the tibia was comminuted to the extent of more than an inch. The patient was enabled to move in his bed at pleasure during the whole period of treatment.

Adolphe C., aged twenty-one, of good constitution and healthy temperament, a soldier of 117th Regiment of French Infantry, received a gunshot wound of the left leg at noon on November 31st, 1870, at the battle of Champigny. On the field, very soon after he fell, Sir John Rose Cormack dressed the wounds and applied a temporary apparatus to the fractured limb. The wounded man was then wrapped in a blanket, placed on a stretcher, and carried the distance of at least two miles over very rough ground, and in a bitterly cold wind. He remained in the "Ambulance volante" for six hours before the journey to Paris could be undertaken. After much jolting and many delays he reached the ambulance, 16, Rue Demours, at 4 a.m. on December 1st. He was then very cold and exhausted. After the administration of warm broth, a full dose of opium, and the bathing of the feet with hot water, he fell into a quiet-sleep for some hours, and was in a pretty comfortable state when Sir John left the ambulance about 11 a.m. on the 1st to return to Champigny, where he remained until the morning of the 3rd, the battle having been renewed with great fury on the 2nd. On the 3rd, in consequence of the pain and swelling of the limb, Sir John relieved it from pressure, placed the fractured limb in a trough, and ordered irrigation. In the afternoon of that day, about seventy-six hours after the wound was received, Sir John showed me the case, and asked me to apply the anterior suspending splint.

I found the limb much swollen. The man was in a state of great nervous excitement, and complained of great pain in the region of the wound. The ball had passed through the left leg,



entering on the inside a little below the insertion of the ligamentum patellæ, passing obliquely downwards and outwards, making its exit about the middle of the opposite side of the leg, thus producing an extensive comminuted fracture of the tibia. The leg probably was raised in the act of running when he received the shot.

The anterior suspending splint was readily applied, and the patient felt great relief as soon as the limb was abandoned to its own weight. He could move himself freely in his bed, raise himself by the aid of the hand-cord to allow the bed-pan to be placed under him, to have his sheets changed, &c., without the least fear of disturbing the fractured limb. The two wounds, the entrance and exit of the ball, were left perfectly free for the daily dressings, the application of poultices, &c. This system of suspension was found very convenient in giving every facility to the surgeon to examine every part of the limb without disturbing the fracture, and without giving any pain to the patient. The bandages were removed and renewed frequently with the greatest ease, and giving great comfort to the patient.

Several abscesses formed and were opened in different parts of the limb. With this exception the patient progressed favourably.

Feb. 18th, 1871. The ambulance in which he had hitherto been treated was about to be closed. The patient had to be moved to the ambulance, 16, Rue d'Agnesseau, maintained by Sir Richard Wallace. The suspending splint was of great service on this occasion in protecting the limb from the effects of any shock or commotion during the transit. A few days after the transit a large piece of necrosed bone, which had formed part of the internal surface of the tibia, was extracted. This piece of bone was at least an inch square, so that the loss of substance, the other fragments of the comminution considered, involved at least an inch of the entire thickness of the tibia. The consolidation of the fracture was, however, found to be sufficient at this time to allow the suspending apparatus to be applied only at night, leaving the limb free during the day, so that the muscles might gradually recover their power, and the joints their flexibility. Two other pieces of necrosed bone were extracted about this time.

The apparatus was entirely removed on the 15th March, and the patient, after walking on crutches for a fortnight or three weeks, was enabled gradually to make use of his limb. One other piece of necrosed bone was removed after he was able to walk. He has now recovered the use of his limb for more than twelve months. There is no shortening of the limb, only a slight indentation where the tibia had been fractured, and a very small fistulous opening still remains, indicating the process of

elimination of some other portion of necrosed bone. The limb is not otherwise deformed, and the man walks as well as he ever did. His left leg is almost as strong as the other; the muscles are developed, and the poor man has been in great distress fearing he may be called to continue his military service.

It will be observed that the patient felt relief directly the limb was left to its own weight. He very soon abandoned the limb altogether to the apparatus, feeling complete confidence and security in the suspension, and was thus enabled to move himself freely in his bed. This security of the patient is accounted for by the action of the splint, which, being placed *parallel* to the axis of the limb, carries the action of the weight of the limb on the bandages from the splint to the axis of the limb, thereby holding the fractured bone steadily in all the movements of the body. The splint becomes a substitute for the fractured bone, the fragments of which are kept constantly in position, so that the process of ossification is not disturbed by any movements of the patient.

The fracture was so comminuted in this case that we may safely say there was loss of substance of at least an inch in length of the bone, yet the limb moved as freely on the suspension as if there had been no solution of continuity at all. Some portions of this comminuted bone having retained their periosteum served with the callus to consolidate the fractured tibia; the remainder became necrosed and was either extracted or thrown off afterwards.

It is important to remark that the usual pain in the heel and the violent spasmodic contraction of the muscles are effectually prevented by the action of the suspending splint. The continued action of extension and counter-extension and all violence in reduction are superseded by the action of the weight of the suspended limb. The fractured bone must inevitably recover its natural length and position to adapt itself to the splint, which has been modeled on the *sound* limb. The muscles of the fractured limb lose their power of contraction by the uniform compression of the bandages which suspend the limb.

The reasons why the anterior suspending splint has not been universally adopted appear to me to be because the principle on which the splint acts has never yet, I believe, been explained, and because it is so difficult to abandon our old friends—the old splints, the double inclined plane, &c.

The splint employed by Professor Smith until he came to France was nearly four inches in width between the wires. That which I employed myself at that time was about three inches wide. Under these circumstances, not being yet aware of the principle of action of the splint, I was naturally inclined to increase the width between the branches, thinking I might



thereby increase the action of the splint. After a time, however, I discovered that the nearer I brought the wires together the greater was the facility I acquired in the application of the splint. *This led me to discover the action of the anterior suspending splint on the axis of the limb—the principle on which, I believe, the whole system is based.* The splint I now use is only one inch and a half wide between the wires, a distance which appears to me to be the most useful and safe in its application. I am convinced that the splint has been brought into disrepute, and been abandoned by many surgeons, because they, like myself, have employed the splint with the branches too wide apart.

Probably the second reason why the anterior suspending splint has not yet been universally adopted is that which I have already hinted at—viz., that we cannot so easily depart from the habits of routine in the treatment of fractures of the lower extremity. It has been supposed that by placing the suspending cord more or less obliquely from the perpendicular line, a power of traction or extension was obtained. I thought so, and have frequently tried it, but always without any advantage. I can account for this now, I think. I asked myself if it is not evident that, as the splint must be attached to the part of the limb above the fractured bone—viz., to the pelvis above the femur, to the femur above the leg, all the traction produced by the oblique suspending cord must act on the pelvis in cases of fracture of the femur, and on the femur in cases of fracture of the tibia. In fact, the action of the oblique cord cannot possibly have any power at all on the fractured bone. On the contrary, by taking the cord from the perpendicular the action of suspension on the axis of the limb is considerably weakened. *The perpendicular cord, in supporting the splint uniformly through its whole length, brings the weight of the limb to act uniformly on its own axis. The cord therefore should never be moved from the perpendicular.* The theory of the action of the obliquity of the cord has most probably been very injurious to the reputation of the anterior splint.

It would only be a repetition of details if I were to apply them to all the surgical cases of the lower extremity. I must, however, add that the anterior suspending splint seems to find its special application in cases of fracture of the neck of the femur, and in the transverse fracture of the patella. In fractures of the neck of the femur, either within or without the capsular ligament, the only recommendation is to fix the thigh immovably on the pelvis, and in fractures of the patella to keep the splint quite straight from the groin to the instep. The straight splint, as above applied in the case of fractured patella, is applicable precisely with the same advantages in suspending the limb after resection of the knee-joint.

I have selected the above case from others equally illustrative of the numerous advantages of the anterior suspending splint over every other apparatus in the treatment of fractures of the lower extremity, but having already taken up so much valuable space in regard to this one, I reserve the remainder for future elucidation should the above description be considered too imperfect or obscure.—*Lancet*, July 20, 1872, p. 73.

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#### 41.—ELEVATION OF DEPRESSED CRANIAL BONE BY PNEUMATIC TRACTION.

By H. W. LARKIN, Esq., Bilston.

The operation of trephining is a hazardous one. Even in the best hands it is attended by no small risk of wounding the dura mater, and thereby inducing fatal encephalitis. The operation is, from this cause, almost as perilous as the condition which it is intended to relieve. So far is this true that in this country, according to Erichsen, about seventy-five per cent. of the subjects operated on die; whilst across the Channel its history is still more unfortunate.

I have recently, in a case of persistent congenital depression of cranial bone in a child, employed pneumatic traction with complete success; and I believe that in many, perhaps most, cases of depressed fracture of the skull, whether occurring in children or adults, this mode of elevation will be found not only more easily practicable, but infinitely safer than that by the trephine.

A short time since I was called to a child a few days old, the site of whose right frontal eminence was occupied by a depression of about an inch and a half in diameter. At birth the child's head had presented abnormally. The maternal expulsive pains had been very strong, the pubic arch somewhat angular, and the child's right temple driven in. Unlike similar depression of the parietals, which is usually transient, being righted, *sua sponte*, immediately after the liberation of the head, this was persistent.

The concavity thus formed was not regular—such as is produced in a thin convex metallic plate when pressure is made on one point of its surface—for in addition to the ridge which formed the circumferential line of the depression, there were two sulci crossing it. The sharpness of the ridge and sulci respectively indicated that the elasticity of the bone had been overtaxed, and that permanent deformity was a probable result. With the view of restoring the bone to its proper shape, a cupping-glass was applied over the depressed portion. Difficulty of exact adaptation was overcome by forming a cell of



ordinary glazier's putty. Exhaustion was effected by a small air-pump. A few seconds proved the experiment successful, for the depressed bone gradually rose and assumed its normal contour. The glass was allowed to remain a few minutes, and was temporarily re-applied the following day. There was no observable constitutional disturbance. The temples are now symmetrical.—*Lancet*, Oct. 5, 1872, p. 490.

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ORGANS OF CIRCULATION.

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42.—THE SURGICAL TREATMENT OF ANEURISM IN ITS VARIOUS FORMS.

By TIMOTHY HOLMES, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

[Now that deep-seated aneurism in the abdomen, and even aneurism of the lower part of the abdominal aorta itself have been permanently cured, surgeons have begun to regard no form of aneurism, however near the heart, as absolutely incurable. The main propositions which Professor Holmes endeavours to establish, in the course of which the following article is the substance of the first lecture, are as follows:]

1. That aneurisms, of whatever form and however near the heart they may be, ought not to be regarded as incurable, but should be made the objects of definite methodical treatment, internal or external.

2. That there is definite proof, from pathological anatomy and from surgical experience, of the curative influence of Brasdor's operation in innominate aneurism, and of its beneficial effects in some cases of aortic aneurism.

3. That arteries may be successfully tied and obliterated without their continuity being interrupted; and that this modification of the ligature, whilst affording much security against secondary hemorrhage, and thus much diminishing the danger of the operation in general, may very probably in future enable surgeons to deal successfully with cases in which it may be necessary to tie the first part of the subclavian (whether on the distal or proximal side of an aneurism) or the innominate artery.

4. That galvano-puncture may be used with at any rate temporary benefit in thoracic aneurism; that its use is not so dangerous as to render further trials of it inexpedient; and that there is good hope that the method may be so far perfected as to make it a safe and regular plan for the treatment of thoracic, subclavian, and other forms of aneurism.

5. That many cases, such as those in which ligature of the artery near to the heart has been resorted to for the cure of subclavian and subclavio-axillary aneurism, may be made amenable to improved methods of pressure.

6. That aneurismal tumours situated even as high as the lower part of the abdominal aorta, those of the mesenteric and other branches of the aorta, and of the iliac arteries, may be treated with success by rapid coagulation of blood under pressure; but that this method is a dangerous one, and should not be used until internal treatment has failed.

7. That there are cases of abdominal aneurism in which Mr. Syme's suggestion of reviving the old operation is worthy of further trial.

Numerous instances are on record in which persons affected with aneurism of the arch of the aorta have, either spontaneously or as the result of medical treatment, been restored to a condition of tolerable health, have lived for a long period of time, and have ultimately died from other causes.

But allowing that it is difficult to obtain complete consolidation in an aneurism of such large arteries, and that the cure when obtained affords no guarantee for permanent recovery, it is still evidently our duty to attempt the treatment of the disease if cure is possible—nay more, if even alleviation and the prolongation of life is possible. Now this latter is not only possible but easy. Judicious medical treatment, with rest systematically enforced, is almost always followed by temporary improvement. And in aneurisms which are growing in any definite direction, and whose growth in that direction threatens important organs, or is likely to lead to rupture of the sac, there is always a chance of stopping such growths for the time, and so possibly preserving the patient's life. I venture to say that this point is hardly enough considered. We think so much of whether an aneurism can be completely consolidated or not that we are apt to forget how often its partial consolidation only is followed by such an alleviation of the symptoms as amounts almost to temporary cure.

It being admitted that cases of growing aneurism, affecting the thoracic aorta, or the innominate, or the roots of the left carotid or subclavian within the thorax, or affecting the aorta and one or more of its primary branches simultaneously, are not to be regarded as wholly incurable, the next question is, What are the means hitherto devised for their treatment? They are three in number—namely: internal or medical treatment; the distal ligature, or Brasdor's operation; and galvanopuncture.

I pass over the injection of ergotine into the cellular tissue around the sac, which has been said in a foreign medical journal



to have been recently recommended by Langenbeck, since that distinguished surgeon has not as yet published his own version of the proposal. The theory of the matter as explained by the journalist seems to me utterly unintelligible, but I have made trial of the method in two cases, with results which were perfectly nugatory; and I have not succeeded in discovering that the experience of others has been more satisfactory. I pass over, also, the plan of introducing a mass of fine iron-wire into the sac, as practised by my late friend, Mr. Moore; since I think no one will deny that it is not more certain than galvanopuncture, while it is far more likely to cause inflammation of the sac-wall. Since Mr. Moore's death, the operation, I find, has been twice repeated—namely, by Dr. Murray of Newcastle, in a case which terminated by suppuration of the sac; and by Mr. Dumville of Chatham, without any definite result. The sac has also been transfixed with fine needles with the same object.

With regard to the internal or medical treatment I shall say very little, since I could add nothing to the admirable exposition lately given of it by Mr. Jolliffe Tufnell. The extreme bleedings recommended by Valsalva have been discarded as calculated to excite the heart to irritable action and to impoverish the blood, thus both impairing nutrition, and so endangering the rupture of the sac, and rendering the blood less fit for coagulation. But the valuable parts of Valsalva's treatment have been retained and methodised. The patient is confined strictly to the recumbent position during a very long period, the most minute precautions being taken to ensure that he can be kept without raising himself once from the bed during many weeks; his food is restricted to the smallest quantity of nutritious solids that can satisfy his appetite, and his drink to the very smallest quantity which can be tolerated. Much stress is justly laid by Mr. Tufnell on this latter precaution, and Dr. Sibson also testifies to the extreme importance of restricting the quantity of fluid to the lowest possible point. The medical treatment of the case otherwise consists almost exclusively in the administration of such narcotics as may be absolutely necessary to calm pain and procure sleep, and such purgatives as may be required to keep the patient comfortable, and avoid all straining in defecation. The great object is to keep the pulse at an equable rate, a little over 60, and free from all excitement. Though Valsalva's extreme bleedings are avoided, a very small venesection may be occasionally serviceable with this view. I would strongly urge any surgeon undertaking for the first time the care of a case of internal aneurism to study Mr. Tufnell's invaluable little pamphlet, which shows how much

may be done for the relief of conditions usually regarded as hopeless, and by how simple, safe, and painless a method.

Brasdor's operation rests upon a sound pathological basis, so far as this—viz., that we have preparations in sufficient number to prove to us that impaction of clot in the distal end of the artery as it leaves an aneurismal sac is naturally (I do not say necessarily) followed by obliteration of that portion of the sac which is distended by the stream of blood formerly passing up the closed artery; that such obliteration, even when it stops short of the consolidation of the whole tumour, may suffice for the removal of all symptoms of the disease; and that precisely the same effect would follow naturally (but here again I do not say necessarily) on the distal ligature of the same vessel. This being granted, it follows that there are some causes of innominate aneurism which may be completely cured by Brasdor's operation.

The condition of an artery immediately after ligature varies much. In some cases an artery which, like the carotid, runs a long course without branches will be filled with clot from end to end (as in Dr. Wright's case); in others, as in Sir W. Ferguson's, no clot whatever will be contained in it. And the same thing is seen at a later period. Sometimes after division only a very small part of it is obliterated; at others the whole artery between the nearest considerable branches is converted into a solid cord. This must have its effect upon the obliteration produced in the tumour by the distal operation. If the blood is so indisposed to coagulate that after the ligature of the carotid no coagulum forms in the artery, it is probable that coagulation will go on but slowly or imperfectly in the sac. In those cases, on the other hand, where the artery is completely filled with coagulum, the blood in the sac will also show a strong tendency to coagulate: first the ordinary blood-clot will form, and this will subsequently become laminated, as happens after the ordinary operation.

It seems doubtful whether we can entertain the same expectation of complete obliteration of the sac from distal obliteration of the arteries when the tumour is partly aortic. An interesting case is here in point, communicated to the Pathological Society (*Trans.*, vol. xx., p. 118) by Dr. Douglas Powell. In this case an aneurism, arising from the arch of the aorta, and involving that artery from above the origin of the innominate to below that of the left carotid, implicated the innominate in its whole extent, and it must have been impossible during life, as far as I can see by the recorded symptoms, to have diagnosed this from a purely innominate aneurism. After death it was found that the carotid artery had been converted into a solid cord from end to end. The right subclavian was also obliterated



at its origin through this, as I am informed by Dr. Powell, must have occurred very shortly before death, since pulsation in the right wrist, which had been absent during the presence of a considerable effusion into the pleural cavity, returned on its removal, and was perceptible till within a few days of death. Here, however, the circulation through the sac was kept up by the stream of blood traversing the aortic and left carotid orifices, and only a little laminated clot was found at its upper part. This deposit of clot was connected in all probability with the coagulation of blood in the carotid artery. And the analogy of the preparations of innominate aneurism above referred to justifies me, I think, in saying that, in an aortic aneurism also, the obliteration of any of the arterics, such as the right or the left carotid, which lead directly out of the sac, will be followed by consolidation in that part of the sac which is distended by the stream of blood formerly passing up the obliterated vessel,

In vol. ix., p. 83, of the same *Transactions*, a very interesting case is related by Mr. Mayo, where an aneurism of the arch of the aorta was found to be far advanced towards consolidation, in consequence of the impaction of clot in the main artery. The tube of the aorta was completely closed by this impacted clot, so that not even air would pass up the vessel. This impaction occurred at the usual situation of the congenital malformation; but the clot, though adherent, could still be detached from the wall of the vessel, showing that the obstruction did not arise from that cause. The aneurism was situated below the three great branches, and the enlarged condition of these arteries seemed to be a step in the formation of the collateral circulation. Death was caused by obstruction to respiration produced by pressure on the lung and trachea. Exposure to vicissitudes of temperature, and habits of intoxication, appear to have had a great share in rendering this obstruction fatal. If the man's habits had been different, Mr. Mayo conjectures with reason, that the aneurism, which was found quite filled with firm coagulum, might have been spontaneously and permanently cured.

Many recorded cases show the severe symptoms which are produced by the distal impaction of clot in cases of aneurism, and the rapid and extensive coagulation which occurs in the aneurismal tumour. Let me call your attention, as an example of this, to a preparation and drawing (from the Irish College of Surgeons) from a case under the care of Dr. Stannus Hughes. I regret that time does not allow me to quote the whole of the very interesting notes which Dr. Hughes has sent me of this case, but I hope they will be fully published elsewhere. Suffice it to say that the patient was a gentleman past middle life, of a

gouty habit, who had long laboured under a pulsating tumour, the origin of which, whether from the aorta or one of its great branches, was obscure. He had much improved in health under treatment, and one day after breakfast was about to go out for a drive, having just said that he felt lighter and better in health than for years past, when suddenly he gave a loud cry, exclaiming that he had an excruciating pain in the chest, and felt so cold that he asked to be laid on the hearth-rug before the fire. Dr. Hughes happened to come in at the time, and found the patient in an agony of pain, pointing to the sternum as the seat of it. In a few minutes this pain disappeared, but he was immediately seized with terrific convulsions, in which he died in a few seconds. The dilated aorta and the left ventricle of the heart were found full of recent coagula. The valves were healthy.

We cannot hesitate to attribute these terrible symptoms to the sudden coagulation of the blood in the arterial dilatation, and the intense pain to the stretching of its coats and the resistance offered to the heart's impulse; and many other cases, to which I could give references if time permitted, show the acute symptoms produced by obliteration of a large vessel with impacted clot, and the rapid collection of blood coagulum which follows on such impaction.

There is, then, sufficient pathological evidence to show that an innominate aneurism may be obliterated by the obstruction of its two distal orifices, and to prove generally that the obstruction of an artery as it leaves an aneurismal sac will in all probability be followed by a deposition of firm laminated coagulum in the portion of the sac connected with the obstructed artery, whereby the growth of the tumour in that direction will be stopped. Now, we must always bear in mind that it is not usually the existence of an aneurism, however large, which is dangerous to life, but the growth of an aneurism, however small, in some definite direction. To stop the growth of a small innominate aneurism towards the trachea, or of a small aortic aneurism towards the recurrent laryngeal nerve, would preserve the patient from a danger which cannot be over-estimated, and would be worth much more risk than is involved in trying the common carotid or the subclavian artery. Again, to line any portion of the sac of an aneurism where it is growing and threatening to burst, or compress important parts, is surely a reasonable object for an operation, even if the operation involves danger, and if the rest of the sac remains unconsolidated.

In combating Mr. Wardrop's proposal for reviving the distal operation, Mr. Guthrie endeavoured to show, from the preparations preserved in our museum, that the distal obliteration



tion of an artery would not cure an aneurism, and I think I ought not to pass over so important an argument, although the data on which it rests are not very extensive. It will be noticed that even in advancing this argument Mr. Guthrie is obliged to admit that such distal impaction is an effort of nature to cure the disease; that is to say, that the disease is partly cured by it, though he did not believe that it could be so entirely.

In treating of the spontaneous cure of aneurism, Mr. Guthrie speaks as follows:—"Two natural processes favouring the cure of aneurism by coagulation have been observed to take place. The first is the enlargement of the collateral branches to such an extent as to enable them to maintain the circulation if the main trunk be rendered impervious. The second is the effort made to close or shut up the lower openings from the aneurism, or those most distant from the heart—an effort which has been hitherto scarcely considered as a natural one, but which is frequently attempted, and often with success." In a further passage he says: "The Hunterian collection supplies several instances of all the openings into an aneurism save the upper one having been closed during life, and I think in sufficient number to establish the fact, that in aneurism of the extremities nature resorts to this method as a part of the curative process; and that although she succeeds in effecting it, still more is requisite to complete the cure by the filling up and obliteration of the tumour. These preparations also show, that when the lower end of the artery has been obliterated the aneurism has not ceased to increase. It is a step which, when once taken, favours the coagulation of the blood; and if a piece of coagulum be separated by accidental violence or other irregularity committed on it by the patient, or by a state of syncope supervening, or by any other means, the coagulation of the whole of the fluid blood in the tumour may rapidly take place."

The preparations he refers to were numbered in the old catalogue 386, 392, and 397, and are at present numbered 1692, 1709, and 1703. Of the last preparation it will be sufficient to say that Mr. Guthrie clearly misunderstood the description which Hunter gave of it, in consequence of that description being, like so many of Hunter's, merely a short note or heading. His meaning was that the injection did not run through the sac into the artery in consequence of that part of the sac which lies next the artery being filled with clot. But a glance at the preparation shows that there is no obliteration of the lower end of the artery whatever. The bougie placed in it shows it opening freely into the dilatation which is part of the sac. I understand Hunter to have meant (as, indeed, the preparation shows to be the case) that cure had commenced by coagulum forming

a barrier between the large sac and the small dilatation behind, which is part of the artery. No. 1709 (formerly 392) has been re-examined, and it is found that the artery, though nearly, is not quite obliterated. Thus, as the process is not complete, we can hardly speculate on its effect, even if the condition of the preparation was such as to admit of a decision on this point, which, however, it is not. The other preparation, No. 1692, in no respect answers to Mr. Guthrie's description as one in which all the openings into an aneurism save the upper one have been closed during life. A slight error is made in the old catalogue, where the obliterated artery is called the brachial, and the aneurism is described as axillary. In fact the artery is the axillary, and the aneurism affected the subclavian, commencing near the point at which the thyroid axis is given off. So that there would be a constant current in the aneurism through the large branches of the subclavian and axillary for anything that this preparation shows. Here, again, only a fragment of the aneurism has been preserved, so that it is impossible to say exactly what effect was produced on the tumour by the obliteration of the axillary; much clot has certainly been deposited in it. But while large branches are left (as was probably the case in this instance) opening out of the sac, it is likely enough that the impaction of clot in even the distal portion of the trunk itself will not cure the aneurism. Even so, however, it may lead to considerable deposition of clot, and may obliterate some part of the tumour, which otherwise would have grown, and possibly burst.

Mr. Guthrie has endeavoured to show that in the cases where Brasdor's operation has proved successful (and that it has been entirely successful in some cases he does not deny, nor that it has in others been partially so), this success has been produced by inflammation propagated from the seat of ligature to the artery, and thence to the walls of the sac. How far Mr. Guthrie would have modified his opinion had the phenomena of impaction of clot in vessels and its results been more fully known to him I cannot say; but I think that anyone who studies his lectures with attention will be of opinion that in this particular his judgment was somewhat misled. It is true that he brought forward the case of Hall, operated upon by Mr. Evans of Belper, and that of the negro operated on by Mr. Montgomery, and succeeded in showing that in both cases inflammation and suppuration in the tumour had followed on the operation. But in Hall's case this inflammation was caused clearly enough by imprudent exposure to accidents in riding and to intoxication after the immediate results of the operation had long passed away; and though the same cannot be said in Montgomery's case, yet suppuration in the sac follows oeca-



sionally after the Hunterian as well as after all other modes of treating aneurism, apparently as the result rather of imperfect coagulation within the aneurism than of inflammation of its parietes. Very probably this is more likely to happen after the distal than the proximal ligature, as it certainly seems to be after Anel's than after Hunter's method; but I confess that I fail to see the radical difference on which Mr. Guthrie so much insists. On the contrary, it seems clear that the coagulation in the sac has been more extensive and complete in cases where no inflammatory symptoms have been recorded.

I conclude therefore that there is *a priori* reason to believe that Brasdor's operation may be successfully applied to some cases of aneurism of the innominate artery, and that when applied to aortic aneurisms it may possibly be followed by partial consolidation of the tumour.

The conclusions on the subject of Brasdor's operation in innominate aneurism, which are justified by present experience, appear to me to be as follows:—

1. That the distal ligature of the carotid alone, or in conjunction with that of the third part of the subclavian, cannot be trusted to produce the complete consolidation of the tumour.

2. That, however, the natural effect of the ligature of the carotid artery is to produce coagulation in the part of the sac directly connected with the mouth of that artery.

3. That this may suffice practically for the cure of the aneurism, when the subclavian portion of the sac is small and shows no disposition to grow.

4. That in other cases, where the mouth of the subclavian artery is previously obliterated by impacted clot, the ligature of the carotid only may effect a radical cure.

5. That for these reasons it is better, in any case which appears to require distal ligature, to commence with the operation on the carotid alone, and afterwards to consider the propriety of securing the subclavian either in its first or third part.

With respect to the justifiability of the operation on the first part of the subclavian artery, I beg to submit the following considerations bearing on the question whether it is possible to avoid in future the occurrence of secondary hemorrhage after the ligature of a large artery. The subject is a new one comparatively; and I must be understood as speaking with all possible reserve on a point where our present experience does not permit us to dogmatise.

In determining the justifiability of repeating an operation which has hitherto never succeeded, the reason of that want of success is, of course, the first subject for our consideration. The ligature of the first part of the subclavian has hitherto failed on account of the almost uniform occurrence of secondary

hemorrhage. There are, it is true, anatomical difficulties and dangers in the operation. In tying this artery for innominate aneurism there would be much risk, in some cases, of wounding or injuring the sac; in others, of finding the vessel diseased and somewhat aneurismal. Still I think most surgeons would allow that, if we could be sure that the ligature of a large artery involved little or no risk of secondary hemorrhage, the operation might be repeated under similar indications to those which would justify any other operation of equal gravity.

Now, an audience such as this need not to be reminded that the ligature of an artery, without its subsequent division, has been an object which has been pursued by surgeons ever since the very first case in which John Hunter tied the femoral for popliteal aneurism.

In one of the earliest cases operated on by Mr. Cline, the attempt was made, by means of a temporary ligature, to produce pressure enough to close the vessel, but without dividing it by ulceration. The same attempt has been frequently repeated, but it does not seem that the usual way of making the attempt is a safe one. This consists in laying on the artery a substance (such as a plug of lint), over which the ligature is tied, and then cutting the knot of the ligature at such a time after the operation as in the judgment of the surgeon is sufficient to ensure coagulation in the tube of the vessel, without producing enough irritation in its coats to destroy their vitality. As this plan has been sufficiently tested, and is allowed to be more dangerous than the common single tight silk ligature, nothing further need be said about it. But the same object has been sought by temporary deligation or acupressure of the artery—*i. e.*, by compressing it with some metallic substance, which is removed when it is judged safe to do so. Allow me to exhibit this specimen, lent to us by the Royal College of Surgeons in Ireland, and showing the obliteration of the femoral by means of the *presse-artère*, and the cure of the popliteal aneurism, for which the operation was performed. The *presse-artères* which have been introduced hitherto do not seem trustworthy; still this preparation shows that an artery may be obliterated, and an aneurism cured, without division of the vessel.

I would refer also to a very interesting case in which my friend Mr. W. Stokes used acupressure or temporary deligation to the abdominal aorta for the treatment of ilio-femoral aneurism, by a wire compressor invented by Mr. Porter. The patient only survived the operation twelve hours, his death being referred to the shock of the operation on a weak fatty heart. The aorta, though held tightly enough to prevent the passage of any water injected into its upper end, showed no trace of injury to any of its coats. In the case, however, in which Mr. Porter



performed a similar operation on the innominate for the treatment of subclavian aneurism, by means of an instrument something like a small lithotrite, after three days of pressure the coats of the artery sloughed and fatal hemorrhage resulted, and when the pressure was removed the pulsation of the tumour recurred. This very valuable and interesting preparation I am also enabled to produce through the courtesy of the College of Surgeons in Ireland. Mr. Porter was of opinion that in this case failure may have been due in part to the comparative clumsiness of the instruments, and has figured in his "Surgical Reports" the more manageable and delicate form of instrument which Mr. Stokes used. The time during which Mr. Stokes's patient survived hardly allows a judgment as to the action of the compressor.

Sir P. Crampton, in the seventh volume of the *Medico-Chirurgical Transactions*., argues that the division of the two internal coats of the artery is unnecessary; and he recommends that the walls of the vessel should be brought into contact by means of a loop, whose tension is to be regulated by a *presse-artère* with just sufficient force to stop the pulsation of the aneurism—the ligature to be removed as soon as, by experiment, it is found safe to do so. Two examples of the successful use of this plan are given, and it has been imitated by various surgeons, while others have invented processes of their own essentially similar. But it has been found that the proceeding is so uncertain, the injury done to the artery so difficult to estimate, and the withdrawal of the ligature therefore so likely to provoke secondary hemorrhage, that it is wellnigh abandoned. Mr. Bickersteth, however, a short time since, used a plan of the kind on the innominate artery.

Since the use of silver sutures has become general, and the absence of ulceration has been noticed which sometimes attends on their remaining even for a great length of time buried in the body, it has been attempted to ensure the same absence of ulceration by tying arteries with ligatures of this substance. But many cases have been put on record showing the uncertainty of this proceeding. The only thoroughly satisfactory case which I am acquainted with, in which the silver ligature has been used to a large artery, was one that occurred in my own practice, and in which I tied the femoral artery for popliteal aneurism. The wound healed, though not without supuration, in less than a fortnight. No trace was ever seen of the ligature, and the man recovered rapidly and completely. He is, I believe, still alive, and was in good health I know a short time ago. In this case a most careful watch was kept for the appearance of the ligature if it should come away; but nothing was seen of it, and I believe that it buried itself in the

tissues of the vessel, and in all probability without dividing the continuity of the latter. In an operation on the carotid of an ass, I found the artery undivided and obliterated six weeks after the application of a silver ligature. The ligature still surrounded the artery loosely. But this substance is, I think, a somewhat dangerous one, for if it is tied at all tightly it will cut the coats of the vessel more deeply, and, I believe, produce more and more rapid ulceration than the silk thread does. Such was the case in this preparation, taken from a patient in whom Mr. Brodhurst tied the femoral with silver wire. In this other instance of ligature of the external iliac by Mr. Pollock the patient died from another cause before the effect of the ligature could be ascertained.

From what I have seen and read, then, of the effect of the silver ligature to an artery, I conclude that its use will in some cases probably preclude the occurrence of secondary hemorrhage, but that it is too uncertain in this respect to be preferable to silk.

A far more reliable material is catgut, and I hope to show you anatomical proof that this substance, when properly prepared, may be used for the ligature of a large artery with perfect success, and without dividing the external coat of the vessel, or interrupting its continuity, while the ligature itself is absorbed; and also to adduce practical proof that the wounds made over vessels so tied often unite by first intention. But I would wish to impress on you the conviction that I entertain, that in such cases it is not merely the material used for the ligature which is of importance, but that the rapid union of the deep parts of the wound is also a necessary condition.

The catgut ligature for the purpose of securing a large artery, was first proposed, I believe, by Sir A. Cooper, and he carried out his own proposal in a case in every way so remarkable that it is astonishing to find it attract so little notice or imitation. In 1817 Sir A. Cooper thus wrote:—"My friend Mr. Lawrence has proposed that the silk usually employed for ligatures should be cut off close to the knot, so as to heal the wound over it. It has occurred to me that catgut would answer the purpose better, and I shall give the result of the trial I have made, wishing it to be understood that I consider the subject at present as undecided, and only as one for future investigation. Catgut employed as a ligature, being more of the nature of the animal matter in which it is embedded, will be more easily absorbed than silk; or even if not absorbed, will be less likely to excite irritation in the parts." He then proceeds to relate the case of a man, aged eighty, who came under treatment at Guy's Hospital on the 15th of October, 1817, for popliteal aneurism, and in whom he tied the femoral artery on the 24th of October,



with "a single ligature applied round the artery, both ends of which were cut off close, and the edges of the wound brought together by adhesive plaster; the substance made use of for the ligature was catgut, which had been previously soaked in water about the temperature of 100°." The wound was not again inspected till the fourth day (October 28th), when it was found "*completely* united"; and on the 7th of November it is noted, "The wound remains perfectly united and free from irritation." Again, on the 24th of November, "No appearance of irritation from the ligature." "In three weeks after the operation he walked in the ward with the aid of a crutch." The notes are continued down to the 17th of December, when it is said, "His health is perfectly good, he walks without the aid of crutch or stick, the swelling is reduced to a small size, and the part at which the incision was made has been and now is quite free from irritation." No wonder that a result so unusual, or rather so unprecedented, to the operation for popliteal aneurism, drew from Sir A. Cooper the following remarks:—"I confess that this case gave me much pleasure. The great age of the patient, the simplicity of the operation, the absence of constitutional irritation, and consequently of danger, and his rapid recovery, lead me to hope that the operation for aneurism may become at some future period infinitely more simple than it has been rendered to the present moment."

Sir Astley's subsequent experience was, however, disappointing. In the 12mo edition of his Lectures, 1829, p. 168, is as follows:—"I thought that a ligature made of catgut would admit of solution, and I tried it on a man eighty years of age, on whom I performed the operation for popliteal aneurism; the catgut ligature was cut down to the vessel, the wound healed over, and no bad symptom followed. The experiment succeeded it is true in this case, but I have used the catgut ligature in three cases since, and did not find it at all superior to the common ligature; in each of the three it came away by suppuration and ulceration as in common cases; it did not therefore succeed. In the old person there was less tendency to inflammation, and that was the reason of its succeeding. I made some experiments on dogs to ascertain the solubility of catgut. I tied the carotid of a dog with catgut ligature; in a fortnight after I killed the animal, and found that the ligature had not been dissolved, but that it had cut through the artery and was situated in a cyst, like that which is formed round a ball, between the divided ends of the vessel, in a quiescent state. Therefore this substance does not admit of solubility, but will remain sometimes without producing irritation. On the whole, catgut ligatures are not at all superior to the common ones. Some animal matter of the form of gluten made into ligature

might do, but this is mere conjecture. At present there is no ligature known which is capable of being dissolved and removed by the absorbents."

In *Cooper's Surgical Dictionary* (art. Ligature) may be found an account of what has been done down to recent times in experiments on animal ligatures. Those introduced into practice by Dr. Physick, in 1814, were made of chamois leather, and the same substance was made use of by Dr. Jamieson, of Baltimore, who 'tied the carotid, the iliac, the femoral, the radial, the posterior tibial, the spermatic, and other arteries with 'buckskin ligature,' and in no instance had secondary hemorrhage occurred; and he states that he has never seen anything of his ligatures, and of course the wounds have generally healed by first intention." The quotation is from the American edition of *Cooper's Dictionary*.

Porta's experience in tying arteries in the human subject with catgut is the most considerable hitherto published. His work gives short notes of nine cases in which the operation was performed by himself, and two in which Guerini was the operator, assisted by him. The arteries were the carotid, femoral, and brachial, each three times, the external iliac and the posterior tibial. In none of these cases was there any secondary hemorrhage; and the results were certainly as satisfactory as those of the ordinary ligature. But I do not see that Porta's practice afforded any conclusive proof of the superiority of catgut as the material for ligature. In the first case (carotid) the patient did well, but the wound took two months to unite. In the second (brachial), the patient died, sixty-five days after the operation, of pneumonia (pyæmic?). The wound had suppurated up to the time of his death, but this suppuration was proved to be only superficial on post-mortem examination. In the third (femoral) the patient died of gangrene in seventeen days; the wound suppurating up to the date of death. In the fourth (carotid) the wound healed soundly and permanently in four days. In the fifth (femoral) the patient died in fifty-five hours. In the sixth (femoral) the patient died on the tenth day of encephalitis. The external wound had healed, but there was a small abscess below the fascia lata, not, however, in contact with the artery. In the seventh (carotid) the patient died in forty hours. In the eighth (external iliac) the patient died on the third day of encephalitis. In the ninth (brachial) the wound was not closed for a month. In the tenth (brachial) the wound did not close for five weeks. In the eleventh (posterior tibial) there was a large wound caused by laying open a traumatic aneurism in the foot. It closed in about six weeks. In none of these cases was the ligature observed to come away, but in all those examined after death, when a sufficient time had elapsed, the vessel was found to have been divided.



It will thus be seen that none of the cases pursued a course remarkably different from the usual one, except the fourth case, in which the wound of ligature of the carotid healed permanently in four days. This is a gratifying result of operating with close-cut ligatures; but it does not, by itself, prove anything as to the catgut ligature, since a similar result has been obtained with silk, as the following quotation from Porta shows—"In Wright's patient, who died five months after the operation on the external iliac, the silk ligature, which had been buried in the wound, was found, on dissection, lying close to the artery enclosed in a small cyst, something like an inguinal gland."

Knight relates that Lizars tied the subclavian for axillary aneurism, April 26th, 1834, on a patient named Hain, with a flax thread, the ends of which were cut short, and the wound was united; the latter healed entirely on the third day, and the patient recovered completely, nothing more being seen of the ligature.

Porta, however, though he strongly recommends catgut as a material for ligature, nowhere, as far as I see, contemplates the occlusion of the vessel without its division; and, speaking of secondary hemorrhage, he says (p. 133): "Hemorrhage being inherent in the nature of the operation and of the organ operated on, and the condition being far more propitious to it in the human subject, we must not flatter ourselves that we shall ever be able to annihilate its possibility by any method, or to reduce it within the limits which we observe in the lower animals."

Mr. Norman, of Bath, also tied the femoral artery with catgut, cut short, March 7th, 1818. The case was successful; but the wound, which "appeared at first to have united in two days, separated. A long and troublesome suppuration ensued, and the wound was not healed till the latter end of April. The ligature was never seen to come away."

Sir P. Crampton also used a ligature of moistened catgut in tying the common iliac artery in the year 1828. The ligature was not cut close; it came away on the eighth day, and the patient at the same time complained of very severe pain. He died of a sudden gush of hemorrhage, from the upper end of the artery, in less than a minute, on the tenth day—*i.e.*, two days after the fall of the ligature. Pulsation had been perceptible in the tumour from the fiftieth hour after the operation, and had so far increased as to justify the suspicion that the ligature had softened and given way. The note on the day on which the ligature came away runs as follows:—"Has the blood returned by the free anastomosis between the internal iliacs?—or has the catgut ligature become macerated and given way? The pulsa-

tion is stronger than could be expected if the tumour were supplied only by the collateral source of anastomosis. The prevalent opinion, therefore, was that the ligature had given way." This opinion was strengthened by the appearances found on post-mortem examination. The artery was not cut through; but its internal and middle coats were completely divided, and there was an opening in it (not described in Sir P. Crampton's account) from which air or water injected into the proximal end of the vessel issued. This opening communicated with an abscess surrounding the artery, and thus a way was afforded for the blood from the artery to the wound.

Now on this case I would remark—1. Had the ligature been properly prepared, it would have held the walls of the vessel together, and prevented circulation through it until its divided internal coats had been sufficiently united to resist the impact of the blood. As it was, the artery, weakened by the violence done to it and the division of a great part of its wall, was doubtless torn by the distension which a great stream of blood caused in passing through it. 2. Or, if the parts around the vessel had become firmly consolidated and united in the first few hours after the operation, they might have answered the same purpose. This seems to have happened with the carotid artery in my case of double distal ligature. Two small depressions are seen close to the track of the ligature, on the inner face of the vessel. One does not lead outside the tube of the artery; but the other leads into a small sac (formed out of the cellular tissue or by the expanded external coat of the artery), and containing a little old blood-clot. If the formation of this little sac had been interfered with by suppuration around the artery, very probably the minute opening might have enlarged and hemorrhage supervened. If Sir P. Crampton had prepared the catgut properly, had tied the ligature firmly, cut it off correctly (both ends close), and had so treated the wound and the patient as to produce speedy consolidation of the deep parts, there would probably have been no hemorrhage.

In a case recently under my own care the patient died on the twenty-seventh day after amputation of the fore arm, of chronic pyæmia, which was, as I suspect, developed before the operation. The stump was healed all but a small opening which led into an abscess, holding about two drachms of foul pus. No trace was found of any of the catgut ligatures which had been applied. These were at least four in number—on the radial, ulnar, and interosseous. Some other arteries might have been tied, but no particular note has been preserved of this. One of the larger arteries, having been divided obliquely, required two ligatures. There was no trace of any irritation around the part where these ligatures had been applied, nor any



pocketing of the matter, or attempt at formation of a cyst. The existence of an abscess in the stump was due evidently to failure in vital power. During the first fortnight the stump had healed with great rapidity; but afterwards, as the patient's powers failed, the closing of the wound was suspended, and the matter became foul. As I always dressed the stump myself, I think I can be certain that the catgut never came away. Besides, in a great part of the stump the edges had healed by first intention, and several of the sutures, which were also of catgut, had never been removed. They had been left to themselves, the part below the skin melting away, and the exposed part dropping off, when this process was complete. [I produce the arteries from this case.]

This is the only opportunity I have yet had of examining the site of an operation of any magnitude at a time which would test the disappearance of the catgut ligatures, except in one other amputation (of the thigh), in which the patient died about the sixth day. The catgut ligature on the femoral artery was found much softened and absorbed, so as to be quite relaxed, and to be drawn off the artery with perfect ease. No secondary hemorrhage had taken place, but there was not much coagulum in the vessel, and I am not certain that secondary hemorrhage would not have occurred had the patient survived. This case, however, happened very early in my use of the catgut ligature, and I doubt whether the gut was properly prepared. It quite coincided with the former case in showing the reality of the absorption of the catgut when buried in the tissues.

Another obvious proof of this same absorption is the fact noticed by Professor Lister, and which I have over and over again verified—viz., that when this same material is used as a suture for a wound, if it be withdrawn in a few days, the part which has been buried below the wound is reduced in volume to a fine thread, its tenuity depending of course on the length of time it has been buried; until, as I mentioned in my case of forearm amputation, the deep part of the suture melts away, and its superficial part drops off. Thus an incidental advantage in these sutures is that they do not require to be withdrawn. Further, the absorption of the catgut ligature is proved by the occurrence of primary union over it, of which I have had at least one very striking instance in my own practice: In a case which occurred some years ago I removed the breast and some axillary glands affected with scirrhus by an incision more than six inches in length. The whole wound united by first intention over the catgut ligatures, several of which had been employed. I kept the woman in hospital for a time to see whether any irritation was set up by the buried ligatures. But

nothing of the kind took place; and I saw her again long after, and ascertained that no irritation, far less any abscess, had ever existed in the neighbourhood of the wound. I mention this case especially on account of the opportunity I had of seeing the patient long after the healing of the wound.

But since the employment of this material primary union after ligature of a large vessel has become extremely common. Thus Mr. Bickersteth says: "During the last three years I have tied the femoral artery five times, the common carotid once, and the common iliac once; and in every case, with one solitary exception, the wound has healed at once, and without suppuration."

In the case in which Mr. Heath lately tied the left carotid on the distal side of an aortic aneurism with carbolised catgut cut short, the wound was not looked at till the eighth day, when it was found to be entirely healed by the first intention. The man is at present in our hall, and you can see after the lecture how simple and perfect the union of the wound has been.

In Professor Lister's well-known tract *On Ligature of Arteries on the Antiseptic System*, he produces evidence which in his belief justifies the following opinion:—"It appears, then, that by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue, and strengthen the vessel where we obstruct it. The surgeon, therefore, may now tie an arterial trunk in its continuity close to a large branch, secure alike against secondary hemorrhage and deep-seated suppuration, provided always that he has so studied the principles of the antiseptic system, and so carefully considered the details of the mode of dressing best adapted to the particular case in hand, that he can feel certain of avoiding putrefaction in the wound. For my own part, I should now without hesitation undertake ligature of the innominate, believing that it would prove a very safe procedure."

Now, I have purposely avoided speaking about the principles of the "antiseptic system" as laid down by Professor Lister,—not that I do not recognise the great interest of the question, but because I have neither the time nor the materials required for its adequate discussion. But I will not refrain from saying, that whether Mr. Lister's explanation of his method of dressing wounds be or be not the correct one, that method is very frequently (I will not say uniformly) successful in producing rapid union, especially in the deep parts of the wound; and if this be granted, which I think no surgeon will deny who will trouble himself to obtain practical experience of wounds so treated; and if it be also granted (which cannot be denied, for it is a matter of demonstration) that the catgut ligature, under



favourable circumstances, will gradually melt away in the tissues without setting up ulceration,—how can it be denied that an artery tied with this substance, and so treated as that the deep parts of the wound may rapidly consolidate, has a chance of that union by first intention which John Hunter and his followers sought after. Such rapid union is impossible in the ordinary method, where a silk thread is keeping up irritation and ulceration in and around the tied artery, and propagating inflammation from the bottom to the surface of the wound.

In my own case of double distal ligature, the wounds were not treated in any special manner. I did, indeed, dress them immediately after operation in the manner recommended by Mr. Lister; but as the man's restlessness during the ensuing night disarranged all the dressings, and the air had been freely admitted to the wounds for many hours before I saw him next day, I discontinued these dressings, and applied merely wet rags. The exact condition of the tied arteries is shown in the preparation and in the drawings before you. You will notice that the carotid artery was obstructed only by a small quantity of clot, which has fallen out of the vessel in making the section. That the obstruction, however, was really complete during life was proved by the fact that only the faintest pulse was at any time perceptible in the temporal artery, and none at all for a calendar month after the operation: so that there could have been no premature softening of the ligature and reopening of the channel of the artery, such as occurred in Sir P. Crampton's case, and in a case where Mr. Spence tied the carotid; and there can be no doubt, I think, that, had the man lived a little longer, a permanent diaphragm would have replaced the clot in the carotid, and its condition would then have resembled that of the subclavian. The latter vessel is closed entirely by an obstructing material, or diaphragm, which is accurately represented in this drawing, and which to the naked eye resembles a fibrous material, but which I have not had examined with the microscope, in order not to interfere with the exact condition of the artery. The diaphragm seems not to be more than about a quarter of an inch in thickness.

This case then affords definite anatomical proof of that which we might have inferred from the experience above referred to of Mr. Lister, Mr. Bickersteth, Mr. Heath, and myself—namely, that it is possible to tie a large artery in the human subject in such a manner that the wound may unite by first intention, and the patient never be in any danger of secondary hemorrhage. And my case shows further, what no experience unconfirmed by dissection could show, that the catgut liga-

ture may be removed by absorption, the vessel remaining undivided.

Now in this latter particular our experience differs from that of Porta. He, as is well known, instituted most laborious experiments on the action of the catgut ligature in the lower animals, besides practising the operation on the human subject to which I have referred; and he came to the conclusion that in all cases a ligature, of whatever kind, tied tightly round an artery, produces a softening of its cellular (or external) coat, and the division of the vessel by a process which he distinguishes from one of ulceration, though the nature of the distinction is not made clear. The exact nature of the process, however, is immaterial to our present purpose. The only point of importance is that in Porta's operation the arteries were always divided, and when divided secondary hemorrhage is of course probable. In my operation they are seen to be undivided, and this is, I believe, the rule when arteries are tied in this manner. Now if the external coat remain undivided secondary hemorrhage is only possible in case of such small apertures as the carotid exhibited in my patient, and these can only give rise to hemorrhage when the tissues in the neighbourhood are suppurating. If the latter remain firmly consolidated the only result will be (as that preparation shows) the extravasation of a minute drop of blood.

The differences which exist between such operations as I have described with the carbolised catgut and those which are related in Porta's great work are obviously two—the different preparation of the catgut, and the more rapid union of the deep parts of the wound. With respect to the first particular, no material can be imagined better adapted for a ligature than catgut long steeped in carbolised oil; it is perfectly tough, perfectly flexible, and perfectly smooth. It excites no irritation, absorbs no putrescent fluid, and though it gradually melts away, yet holds the artery firmly enough to close its tube permanently. Having used it in all large operations for about two years I can testify to the latter fact, and I can say that I have never seen any sign of irritation produced by it, nor witnessed the escape of any of the little knots from the wound, and that I hold it to be a very much better and more convenient hæmostatic than torsion, and infinitely superior to acupressure. In fact, I have found it to possess all the comfort of the silk ligature without its disadvantages.

If catgut is imperfectly prepared, it has not all these good qualities. If it be softened too much (as may easily be done when it is steeped in water), it will relax, and not hold the vessel permanently closed. If it be too little softened (as may easily be done when moistened in the mouth), it is very probably too harsh and irritating, and may be absorbent.



But I do not imagine that catgut, or any other ligature, can be applied to an artery with perfect success, by which I mean, so as to close its tube yet not interrupt its continuity, unless the artery itself is kept as much as possible free from inflammation—that is, unless the tissues around become rapidly coagulated by first intention. An artery exposed in the middle of a suppurating cavity will, I believe, always soften and give way; and it was in all likelihood because they failed to secure this prompt union of the deep parts of the wound that Porta in most of his experiments and operations, and Sir A. Cooper in the operation which he performed after his first successful one, failed to obtain this perfect result. At any rate, the fact that such results are possible must have its bearing on the question whether Brasdor's operation in its integrity is or is not applicable in aneurism of the innominate artery. For I think we have in the facts I have produced ample evidence that if the carotid and first part of the subclavian are obliterated, the coagulation of the entire tumour is the natural result; while the ligature of the carotid and third part of the subclavian, though it may possibly produce a cure, cannot be certainly trusted to do so.

In those aneurisms, then, of the innominate artery which appear to be advancing after distal ligature of the carotid, the surgeon is justified, I think, in proposing to tie the subclavian in the first part of its course.—*Lancet*, June 8, July 13, Aug. 31, and Sept. 7, 1872, pp. 783, 35, 287, 325.

#### 43.—ON THE TREATMENT OF ANEURISM.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

[Up to the present time aneurisms have only been treated by arrest of the circulation in the vessel leading to the aneurism, (afferent vessel). There are some aneurisms, however, in which the afferent vessel is out of reach, and such cases have been hitherto incurable. It has, however, been discovered that to produce a firm coagulum in the sac, it suffices to arrest the circulation through the efferent vessel.]

Before we pass on to consider what our art can do in such cases, let us stop briefly to see what is to be learnt from the processes of nature herself; and, without going into details, I may state that sufficient evidence exists to justify Poland's assertion (*Guy's Hospital Reports*, 1871) that "indirect compression on the distal side of the sac—on the efferent vessel—has been met with as a spontaneous source of cure in some rare instances of aneurism of the subclavian artery." Hodgson's case of a soldier is the best one to illustrate this point. It was

a case in which a large aneurism of the aorta coexisted with a small one of the subclavian artery, and the aortic aneurism had so compressed a portion of the subclavian artery beyond the subclavian aneurism as to have caused its obliteration. The small aneurism was nearly filled with layers of coagulum, and the subclavian artery, from the point where it emerged from this little sac, was completely filled with a firm ligamentous substance. You may accept this case, therefore, as a typical one of a natural cure of an aneurism by pressure upon the efferent artery; and you must have remarked that the cure had been effected by the same pathological process as has been recorded in all cases of cure by obstruction of the efferent—by the formation of clot in the aneurismal sac.

Let us now proceed, then, to see how the surgeon can imitate this natural cure; what hints we can glean from the contemplation of the fact that an aneurism can be cured by the occlusion of the efferent artery—for there is surely no reason why art should not imitate natural processes—why surgeons should not so assist nature to effect a cure—why, mechanically, the surgeon should not so apply pressure in one of its forms to the distal side of the aneurismal tumour as to close the efferent artery, and thus artificially cause the deposition of a fibrinous clot in the aneurismal sac similar to that already described to have been deposited by means of the accidental pressure of the subclavian artery beyond the subclavian aneurism; and I think I shall be well able to show you that such a result can be obtained. The first evidence I propose to adduce shall be a pathological one, the second clinical.

The pathological is represented by this preparation: a more beautiful and valuable one I do not know, for it shows as clearly as facts can show that an abdominal aneurism can be mechanically filled with clot, even to its occlusion, by means of mechanical pressure applied to the efferent vessel for only a few hours; and that such a clot seems to be as well formed and of a like nature to that deposited by the obstruction of the afferent artery. It suggests, also, a mode of treatment by which many aneurisms may be treated that are beyond the reach of other surgical means, and well supports the theory and practice which were suggested and applied by Brasdor and Wardrop—namely, the application of a ligature to the distal side of the aneurism.

The details of the case have been laid before the Medical and Chirurgical Society (April 8, 1872.) On the present occasion it will be enough for me to tell you that the aneurism was one of the celiac axis, and that the consolidation you see in the sac was the consequence of pressure by means of Lister's abdominal tourniquet for sixteen hours, the man dying from peritonitis, the direct result of some contusion of the peritoneum from the pressure of the tourniquet.



I will now give an example of Wardrop's operation, or the application of a ligature to the subclavian artery for aneurism of the innominate, the carotid being left alone—Wardrop's operation consisting in the application of a ligature on the distal side of the aneurism, leaving a large arterial trunk between the aneurism and the point of ligature, whilst Brasdor's operation consists in the application of a ligature to the efferent artery itself beyond the aneurism, with no branch between. It was suggested by Brasdor, but first successfully applied by Wardrop to the carotid artery, and is doubtless a good operation. I shall give you some facts by-and-by to support this assertion.

*Aneurism of the Innominate Artery—Ligature of Subclavian (Wardrop's Operation)—Recovery.* [Reported by Mr. E. Hicks.]—James S., aged thirty-three, was admitted into Guy's Hospital, under Mr. Bryant's care, on August 7, 1871, with an aneurism of the innominate artery. He was a healthy carter, a resident of Bolton, in Lancashire. He had always had good health, never having had syphilis or rheumatism. Eighteen months ago, when lifting a heavy box with his arms raised, and supporting it with his neck and shoulders, he felt a pain in the right side of his neck. He thought little of it; but within a few days he felt a pain in his right arm, and a sensation after using it as if his hand was asleep. He then observed a swelling above the right collar-bone, which pulsated. He went into the Manchester Infirmary for five months, and subsequently into the Bolton, where a weight of lead of about two pounds was fixed over the part. This did, however, he says, no good.

On admission, a soft expansive pulsating swelling, rising from behind the right sterno-clavicular joint is to be seen. It clearly involves the sternum and sternal end of the clavicle. It runs well out of the chest, and pulsates freely. The patient has a slight cough, and at times difficulty of deglutition. There is no distinct aneurismal bruit. The second sound of the heart is very loud in the tumour. The right radial pulse is small. The man was ordered to keep his bed and to have ice applied to the tumour.

On August 19, Mr. Bryant applied a ligature to the right subclavian in the third part of its course—using a carbolised gut ligature. No difficulty was experienced in the operation; a large vein, which was wounded as it entered the internal jugular, was ligatured. The wound was closed and covered with lint soaked in the compound tincture of benzoin, cotton-wool covering the arm. He passed a good night after the operation. He has still a little cough. No rise in temperature.

August 22. Slight pulsation to be felt in right brachial artery; none in radial.

23rd. Slight bleeding took place in the wound, and again six hours later. It was stopped by ice.

24th. All the dressings were removed yesterday. Wound discharging (from the only spot that has not healed) some broken down blood. Ice to be kept on. The man says he feels quite well; takes food kindly.

25th. The wound discharges very little now. The tumour is certainly smaller and harder; it pulsates very feebly.

September 6th (seventeen days after the operation). A sphygmographic tracing was taken from the radial artery.

11th. The man was up, the parts having consolidated; wound healed. Tumour decidedly smaller and harder, and pulsating very feebly.

22nd. The man left the hospital convalescent. He was to return after the lapse of a few months to have the carotid ligatured should the aneurism have increased.

I must not, however, rest satisfied with the mere personal experience I have to give—it is good, there can be little doubt, and enough to warrant the inference that the forms of practice indicated are worthy of thought and of practical application; but I can back it up by some good recorded experience of others. I can refer you to a case of Edwards's, published in the *Lancet* for 1858, in which distal pressure was applied to the carotid and subclavian arteries of a lady, aged fifty, for aneurism of the innominate. It was applied by means of an instrument (for a description of which I must refer you to the original paper) kept on for three months; and at the end of that time the tumour, from having been the size of an apple, and soft, came to that of a nut, and hard, all pulsation in it having ceased with all other symptoms.

The case of aneurism appeared to have been cured by means of pressure applied to the efferent vessel.

I can tell you also that out of seven recorded examples of Brasdor's operation upon the carotid for carotid aneurism, four recovered—one of Wardrop's, one of Bush's, one of Wood's (of New York), and one of Colson's; out of three for carotid and innominate aneurism, one—Evans's case—was alive thirty-four years after; out of fourteen cases of ligature of the carotid on the distal side for innominate aneurism, five were cured—one of Wickham's, Fearn's, Scott's, and two of Pirogoff's; and of the two cases on record similar to that I have just recorded of my own, in which the subclavian was ligatured for innominate aneurism, one—Wardrop's case—was alive two years after, and Broca's survived six months.

I must tell you, however, that five out of six of the cases of simultaneous distal ligature of the subclavian and carotid arteries for innominate or aortic aneurism proved fatal, although Heath's case lived four years after the operation.



Now, I think you will agree with me that these facts, pathological as well as clinical, are enough to prove that the treatment by pressure, and, on this failing or being inapplicable, by ligature upon the efferent artery of an aneurism, is an operation which holds out very good hopes of securing a fair amount of success—of success, also, in a class of cases that, under other circumstances, must be left alone, for they are beyond the reach of the surgeon's art. I mean certain examples of abdominal aneurism and of aneurism of the innominate and first part of the subclavian and common carotid arteries. In aneurism of the common iliac and external iliac it may also be of value.

There is, however, one other point which the successful treatment by pressure upon the afferent and efferent artery of an aneurism brings prominently forward, and that is the use of some other expedient by which the circulation in the vessel may be temporarily arrested; for although digital and instrumental pressure are good, they cannot always be employed, and it would be well if some other practice could be adopted which would act upon the same principle. John Hunter doubtless had this in mind when he adopted the practice of applying a temporary ligature to an artery for the cure of aneurism; and Sir A. Cooper and Mr. Travers tried to further it by some experiments upon animals. But the practice never found its place in surgery, and yet there must surely be something in it; for if by the mechanical pressure of a finger or instrument upon an artery leading to or from an aneurism the mechanical closure of that aneurism may be secured by means of coagulum, surely the same end may be as confidently looked for by the use of some temporary ligature or other occluding medium.

It is possible that in Lister's antiseptic catgut ligature this temporary occluding medium may be found; for by means of it it appears that an artery may be occluded as effectually as by any other ligature, and that, as the gut subsequently dissolves, no irritant is left around the artery to set up an ulcerative action for its discharge that may neutralise the good the application of the ligature had effected.

With the same object the artery constrictor I now hand round, of Dr. S. Fleet Speir, of New York, appears to me to possess a value of no little importance. I do not say that the instrument itself is perfect, for I do not think it is; but the principle upon which it acts is a good one, and worthy of adoption. The instrument with an essay obtained a prize from the Medical Society of New York in 1871 (*vide Medical Record*, New York, April 1, 1871). I have made many experiments with it on the dead subject, and find that it does all its inventor claims for it; it produces a division of the inner and middle coats of the artery and their valvular incurvation or invagination—in

fact, doing for an artery in continuity what torsion does for a divided one. I have used it but once on the living subject—upon the brachial artery—and it answered well.

I cannot allow myself to say more upon this important point; but I feel strongly that if from the pressure of an artery—afferent or efferent—for only a few hours an aneurism may be cured, the same result may not unreasonably be looked for by the adoption of other temporary means of occluding a vessel, such as some temporary ligature, catgut or otherwise, or the application of some instrument like, or acting on the principle of, that of Dr. Spiers; for I can well see that by the use of such an instrument or means many cases of aneurism are to be treated in which pressure cannot be employed, and the use of the permanent ligature which has to ulcerate through a vessel, and all its dangers, may be dispensed with altogether.

I believe that in this direction much will be done in the next few years, and I leave the thought to some of the active minds I see before me for future development.—*Medical Times and Gazette*, July 27, 1872, p. 87.

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#### 44.—A CASE OF POPLITEAL ANEURISM CURED BY PRESSURE.

By Dr. JOHN K. BARTON, Surgeon to the Adelaide Hospital, and Examiner in Surgery, Queen's University, Dublin.

In undertaking the case of a popliteal aneurism by pressure, the object to be aimed at is not only the solidification of the tumour, but also to accomplish this (1) in the shortest possible time; (2) with the smallest possible suffering to the patient; and (3) without injury to the integument.

The plan adopted in the following case fulfilled these indications very well.

Iodide of potassium was given during the entire time of the treatment; how far this may have influenced the solidification of the tumour must remain very uncertain; but it is a curious fact that a drachm of this salt could be taken daily, for ten days, without producing any unpleasant symptoms.

The notes of the case were taken by Mr. Heath, resident pupil, who watched the case, and carried out the treatment with the greatest possible attention.

Thomas Latimar, a shoemaker, 32 years of age, presented himself at the Dispensary of the Adelaide Hospital, upon the morning of May 24, 1872, to obtain advice for a swelling in the ham of the left leg.

Dr. Ward having examined him, at once diagnosed the case one of popliteal aneurism, and sent him into hospital, where he was admitted under Dr. Barton's care. Upon examination, the



following facts were noted :—The patient is a slight well-formed man, of middle stature. In the left popliteal space a pulsating tumour can be felt, rounded in shape, and about the size of a hen's egg—the pulsation is very strong and distensile, being nearly as forcibly felt on either side as directly behind. Pressure on the femoral artery at once causes the pulsation in the tumour to stop. No bruit can be heard in the tumour; the posterior and anterior tibial arteries can be felt, same as those of the other limb. The tumour in the ham is painless, and the ordinary colour of the skin. A soft murmur can be heard, but indistinctly, accompanying the second sound of the heart; most audible at the base of the organ.

*History.*—Had rheumatic fever about ten years ago; was in hospital about eighteen months ago for dropsy following scarlatina; after recovering from which, suffered from palpitation of the heart. About six months ago, began to feel a stiffness of the left knee, and in a month from the time he first felt the stiffness, he noticed a tumour behind the knee, and at the same time experienced a dull aching pain about the knee, and great difficulty in fully straightening the limb, which felt most comfortable semi-flexed, and, as his trade allowed it, was usually kept in a bent position. This pain got better when the limb had been kept at rest for some time, but returned severely, upon the patient indulging rather largely in porter, and subsequently, whenever stimulants were indulged in, the pain came back, and each time it returned it was more intense than before. He never received any hurt, wound, strain, or other injury to which he could refer the appearance of the tumour. There are cicatrices in the groin of a suppurating bubo, but no signs of syphilis. The patient has cramps in the calf of the leg frequently. The affected limb measured round the knee half an inch more than the other.

Saturday, May 25th. He was ordered to take three times a day twenty grains of iodide of potassium and one drachm of the syrup of the iodide of iron, and to remain strictly in bed; the hair was ordered to be shaved off from the left groin, and the skin in this region to be bathed several times a-day with spt. of camphor.

Monday, 27th. The following arrangements were made for applying pressure:—A Skey's tourniquet, adapted to press the artery as it crosses the ramus of the pubis. A clamp, invented by Dr. Barton for another case, was applied about the middle of the thigh to press the artery in Hunter's canal. The object of the second instrument was to relieve the groin occasionally, and when it became necessary to relax the pressure at the groin, still to command the circulation through the tumour. Several students volunteered to sit by the patient all day, and see that the pressure was steadily maintained.

The pad of Skey's tourniquet was screwed down, and the pressure commenced at 11.10 a.m. No pulsation whatever in the tumour; no pain complained of. At 1.15 the clamp was tightened, and the tourniquet loosened, as the pain of its pressure was much complained of. At 2.30 the tourniquet was re-adjusted, the clamp being painful. Patient complained of cramp in the calf of the leg, and shooting down to the foot. At 5.30 a hypodermic injection of one-fourth of a grain of morphia was given, with the effect of enabling the patient to bear the pressure without any complaint of pain up to five minutes of 11 o'clock p.m., when all pressure was removed for the night. Scarcely any pulsation was to be felt at this time in the tumour.

Tuesday, 28th, 9 a.m. Pulsation in the tumour distinct; but not at all so violent as before the treatment was began. Skey's tourniquet re-applied at 11.15; no pain complained of; all pulsation at once ceased in the tumour. Pressure by means of this instrument was kept up all day, the patient complaining of occasional pain, which was relieved by a re-adjustment of the pad. At 11 p.m. he complained of severe pains. The instrument was then loosened, and the tumour was found upon examination to remain quite pulseless. The instrument was, however, re-applied upon a new spot; it was left on all night, but became displaced during sleep, so that probably very little pressure was made during the night.

29th. Upon examination at 10 a.m., the instrument having been entirely removed for three hours, a slight thrill only could be felt in the tumour.

Under these circumstances, a weight of  $3\frac{1}{2}$  lbs. was adjusted so as to rest on the artery just before it crossed the pubis, and consequently a little above the spot pressed upon yesterday by the pad of the tourniquet—a bandage secured the weight in position. This was sufficient to arrest the circulation in the aneurism, for no thrill whatever could be felt after it was put on. The patient felt no pain from the pressure, a full or tense feeling in the calf of the leg being the only uneasiness complained of. He passed the day and following night without any re-adjustment of the weight being necessary.

30th. Upon removal of the weight this morning all pulsation in the tumour was found to have ceased. All pressure was now removed; a chloride of lime lotion was applied to the groin where the skin was discoloured and a little vesicle had formed.

The limb was kept at rest by means of a long outside splint. The clamp being put round the limb, but not tightened, but ready to be screwed tight if any pulsation returned in the tumour.

31st. The clamp had not to be tightened at all yesterday or



last night—tumour remaining quite pulseless. The collateral circulation is well established, a large branch pulsating in the inside of the knee.

June 1st. Skin in the groin, which had suffered from the pressure, is fast recovering itself; tumour remains pulseless; foot and leg bandaged with pad of cotton wadding in popliteal space; and foot secured to end of bed to prevent motion; splint removed.

June 6th. Allowed up—not to walk—to go about in a wheeled chair.

Patient is well in every respect.

As seen by the above notes, 60 hours was the full time during which pressure was applied in this case. Of these 60 hours 24 only were occupied by firm pressure; the remaining 36 by a light pressure, sufficient to check circulation in the tumour; but not enough either to produce much pain or to endanger the integument. That the popliteal artery no longer conducts the blood to the leg is proved by the fact of the distinct enlargement of the collateral channels; and it, therefore, may be presumed that the pressure employed was sufficient to arrest the circulation through the vessel altogether.—*Dublin Journal of Medical Science, July, 1872, p. 1.*

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#### ALIMENTARY CANAL.

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#### 45.—ON STRANGULATED HERNIA.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[Whilst on active duty at the hospital, Sir James Paget operated on about a hundred cases of strangulated hernia. The varieties of hernia, their complications, and the different conditions of people in whom they occur, make so great a variety of cases, that it would need a tabulation of at least a thousand to obtain conclusions of real value.]

Our first subject may be the grounds for determining whether a strangulated hernia, or one supposed to be so, is to be submitted to operation. Speaking generally, one may say that when in a case of hernia signs of strangulation are present, and reduction by the ordinary means cannot be accomplished, the operation for reduction should be at once performed. But then, what are the sufficient signs of strangulation, and what are the ordinary, or, better, the reasonable means for reduction without operation?

In all well-marked average cases, that is, in such as are neither slight nor very severe, neither (as some divide them) acute nor chronic, these following may be regarded as the signs of strangulation justifying operation. The hernia, usually reducible, or now first formed, cannot be reduced by reasonable means. If not recently formed, it is larger than usual, tense, firm, or even hard; without impulse, without resonance, painful, and tender on pressure, especially at its mouth and neck. The bowels do not act, though they may often be felt contracting, and may cause much colic and spasmodic pain, especially at the navel and the pit of the stomach. With this pain there is commonly some tenderness, with a feeling of tightness in the abdomen, especially about the navel, and between it and the seat of the hernia. The patient is often sick, vomiting nearly all the food and drink that he swallows, and besides, a quantity of gastric and biliary secretion, or of the contents of the small intestines more or less diluted. The pulse and respiration are usually quickened and rather feeble; the patient feels and looks restless, low, and miserable—or, as it is called, “anxious.” He cannot sleep or eat; and the hands and feet are apt to become cold and shrunken and dusky.

Whenever all these things are observed, and when they remain after such reasonable attempts at reduction without operation as I will presently speak of, you may hold that the operation should be done without delay. Much more, if possible, should it be done if these things be all worse than I have described. When the integuments over the hernia are inflamed, thick, sodden, and ruddy, or emphysematous; when the whole abdomen is swollen, tense, and tender; when the vomit is just like the liquid fæces of the ileum; the pulse very rapid, feeble, and small; the skin cold and dusky and clammy; when the patient is dim in sense and mind, or in an anguish of misery with retching and hiccough; when all or the greater part of these elements of what the old writers called a *miserere* are combined; then, without trying any other method of reduction, you must instantly operate, though you may have only the slenderest hope of doing good, and a serious fear of seeming to do harm.

Thus far, one may speak very positively. In the cases which I have sketched, there are the sufficient signs of strangulation; and if the hernia cannot be safely reduced without operation, the operation must be done. But you will not see many cases without seeing some in which, although the hernia may be irreducible, yet the signs of strangulation are very slight, obscure, incomplete, or in some other way not sufficient to make it nearly certain that the operation is necessary. It is an easy rule for all these cases that, whenever you suspect that hernia is strangu-



lated, you should operate. If you will follow this easy rule, you will do some very bad surgery; you will kill a few patients whose lives you ought to save; and you will make many ill for two or more weeks who might be well in as many days or hours. You must avoid the easy rule, and learn the hard one of discriminating the cases that require operation. You must learn to discriminate those on which the operation must be done at once, without any previous attempts at reduction, and those in which before operation one or more attempts at reduction should be made with chloroform or other helps. For the purpose of such discrimination, let it be your design, in each case of strangulated hernia, not to choose any measure for its reduction till you have fairly weighed the signs of strangulation of which I have spoken. For convenience of thinking, you may divide them into the local symptoms, including all the characters of the hernia itself, and the less or more remote or general symptoms, especially the inactive bowels, the state of the abdomen, the vomiting, the pulse and respiration, and the general condition. I will try to tell how each of these, in its several degrees, may be estimated.

1. First, of the local symptoms, the irreducibility of the hernia, its unusual size, its tension or hardness, and the others which I enumerated, a few rules may include all that I can tell of these. It will be convenient to speak of irreducibility last; for, though it may seem as if it should be the condition decisive for operation, it is really a fallacious sign of strangulation; and in some instances, no trial should be made to test its existence. As for the other local symptoms, their presence, in even a marked degree, is not decisive of strangulation, and is not sufficient to prove the need of operating when the remoter signs are not present. For the local symptoms may be found when a hernia, or its sac, is acutely inflamed, though not strangulated. In this state, which may follow injury, or even arise spontaneously, a hernia may become quickly larger than ever, firm and very tense, without impulse, very painful and tender, hot and red, and not at once reducible, by any fair means. The sac may suppurate, the integuments may slough; and yet there may be no strangulation and no need of an operation. I have, indeed, only once seen this sloughing of the integuments over a hernia; but the case is not likely to be without parallel, and was a glaring instance of the fallacy of the local signs of strangulation.

A very stout elderly lady had a large umbilical hernia, which became painful, tender, and irreducible; her bowels did not act, and she felt sick, but did not vomit. There appeared no urgent need for operation, and she was one in whom an operation was not to be undertaken lightly. But, after three days'

watching, during which the local, but not the remoter, signs had somewhat increased in severity, I found that a large portion of the thin integuments covering the hernia had rapidly sloughed. I operated at once, though with little hope of doing good; for I supposed that the contents of the sac must also have at least partially perished. But they were not even severely strangulated; the mouth of the sac was too small for their return, but they were not tightly girt, and the intestine was only moderately congested. The sloughing of the integuments seemed due to inflammation in a very feeble person, and an ill-nourished part; and with the same feebleness the patient died on the day after the operation.

You may find, then, and not rarely, that the local characters usually present in a strangulated hernia may be imitated in an inflamed hernia which is not strangulated. And, though very rarely, many of the remoter signs—the constipation and the vomiting, the quickened pulse and breathing, and the rest—may exist when a hernia is inflamed, but not strangulated. How, then, can you discriminate? Generally thus: in the inflamed hernia, without strangulation, the local signs precede, and greatly predominate over, the remoter and general signs; while, in a hernia which is inflamed after becoming strangulated, the remoter and general signs will still predominate over the local, and the history will tell that they preceded.

These means of discrimination, however, will not always suffice. You will meet with cases in which you will be uncertain whether the hernia be only inflamed and irreducible, or strangulated and inflamed; but in these cases you must not be uncertain of your practice. If you cannot very easily reduce the hernia, you must operate. The risk of operating on a hernia which is inflamed and not easily reducible is very small, in comparison with the risk of leaving one which is inflamed and strangulated; and even if you can find reasons for waiting, it must be with the most constant oversight, for an inflamed and irreducible hernia may at any time become strangulated, and will certainly do so if not relieved by rest and other appropriate treatment.

For a second rule: if the remoter signs of strangulation be present, the local signs are urgent for speedy operation in the same degree in which they are marked, or in even a greater degree; for severe strangulation is often associated with slight local symptoms.

But, to judge rightly from the local symptoms, each may need to be carefully weighed as evidence for the necessity of operating, or of adopting other methods of reduction.

In reference, then, to the chances of reducing a hernia without operation, it is a bad omen when one has quickly come down



much larger than ever before. A great majority of patients give this as the beginning of their troubles. Sometimes they tell that, while making a great effort, they felt the descent come larger than ever; that they felt some pain or more than usual increase in the hernia, and could not reduce it in the customary way. More often the larger descent has happened without apparent cause. While the patient was sitting, or in bed, or quietly walking, the descent has occurred; but, perhaps, most frequently it has seemed connected with some diarrhoea, or colic, or spasmodic pain, or with some kind of turbulent movement of the intestines, for some hours, or a day or two, before the descent. In some cases, moreover, the unusual size of the hernia is attained at once; in others by gradual increase. In some it very quickly becomes painful; and these are rather less likely to be reduced than those in which pain follows more slowly.

I cannot give you any satisfactory explanation of these unusual descents of herniæ or of their becoming strangulated; but let them teach you not to be deceived by any notion, that some unusual or startling event is necessary as a cause for the strangulation of an old hernia. And I believe it may be held as a safe rule in practice, that the more a recently descended hernia exceeds its usual size, the less is the probability of its being reduced without operation; and I think that the probability becomes the less, the more the size of the hernia continues slowly to increase; for not a few of those that have suddenly become very large, and then have not increased, may be reduced without operation, if the patient be put under chloroform soon after the descent.

Similarly, when general signs of strangulation exist, the harder and more tense a hernia is, the less is the chance of reduction without operation. The hardness may be due to any one of several conditions; but to whatever it may be due, it is an untoward sign. It is especially so, if the hernia be a small one. In large herniæ, the hardness may chiefly be felt at and near the neck and mouth of the sac, especially in inguinal herniæ; and you must take care not to be deceived by a sac which is soft and flaccid everywhere except at its mouth; for there may be strangulated intestine in the mouth of the sac, though the rest contain only soft omentum or fluid not sufficient to distend it. Nay, you must not let even a wholly soft condition of the hernia, or an open external ring, weigh down against the well-marked general signs of strangulation; for the piece of intestine at the mouth of the sac may be too small to give a sensation of hardness, or the whole hernia may be omental.

Again, if the general signs of strangulation exist, the more painful and tender a hernia is, the less, speaking generally,

is the chance of reduction without operation. But here it is to be observed, that this rule holds less for recent than for old herniæ. A recent hernia may be horribly painful, apparently because of the tension of the stretched fibrous tissues about the sac's mouth. Yet such an one may commonly be reduced with the help of chloroform; but an equally painful old hernia, or one that has slowly become thus painful, may require speedy operation. And the operation must be all the more certainly done when, together with any of the remoter signs, there are other local signs, such as inflammation of the coverings of the sac, suppuration, emphysema, or the like. These are imperative for operation, without any previous attempt at reduction.

And for another rule: if the remote signs of strangulation be well marked, and the hernia cannot be otherwise reduced, you must operate, though there may be no marked local sign at all. Or, even beyond this, if the general signs of a strangulated hernia be present—the constipation, vomiting, and others—and there be anywhere a swelling which may be a hernia, though it seem not likely to be a strangulated hernia, the operation must be performed at the seat of that swelling.

Reasons enough for this rule may be found in the many cases in which the local signs of a strangulated hernia are so little marked that the patient, having his attention spent on the misery of his vomiting and epigastric pain, and other symptoms remote from the hernia, says nothing of the hernia itself. In not a few of my recorded cases, the hernia had been overlooked for a day or more; and the patient had been treated for spasms, colic, dyspepsia, or some other imitated disease, while the hernia was obscurely becoming hopelessly strangulated.

This last rule, of operating though there be no local signs of strangulation, may lead you into trouble; into the trouble and discredit of performing an useless operation, and seeming to do much more harm than good. But this you must face; it is just in instances such as this that surgery must incur the risk of seeming to do harm rather than miss the opportunity of doing good. I have operated thus uselessly in three cases. One was an irreducible umbilical hernia, in which there was no strangulation; but vomiting and other signs of strangulation, caused by (I believe) gall-stones. Another was a simple femoral hernia, with an internal strangulation of a piece of intestine far away from it. The third was an inguinal hernia with the same complication. All the patients died, and my operations seemed worse than useless failures. But you must face this risk of seeming wrong.

You may have to go further than in these cases; and if, for instance, a patient have two herniæ that are irreducible, and signs of strangulation, and you cannot tell which is strangu-



lated, you must operate on both. I saw Mr. Stanley do this; and no one could blame him, though, when death followed, it was found due to an internal strangulation distinct from both the herniæ.

These are the chief rules, so far as I have been able to learn, according to which you may use the local symptoms of a supposed strangulated hernia as a part of the evidence for determining for or against an operation for reduction. And to these it may be briefly added that the local symptoms are generally less severe in the old than in the young; in old hernia than in new; in omental hernia than in intestinal.

[In his second lecture, Sir James Paget speaks of the trials at reduction that may be made, or may not be made, before operating.]

For a general rule, let me repeat, your first examination of a patient with a hernia supposed to be strangulated should not be with a design to reduce it at once, but rather with a design to make out what shall be done; what are the chances of reduction without operation; what helps shall be used to obtain it, if it be deemed desirable. Of course you may reduce the hernia, if reduction at once be easy; but do not go on trying if it be difficult. If the case be a bad one, you must first decide whether reduction without operation should be even attempted; and, if the attempt is to be made, what and how much it should be. For this decision, here are some general rules, which I find illustrated by my cases.

In very bad cases—as, for instance, when the patient vomits faecal matter and has peritonitis, or is in collapse, with a small rapid pulse, hiccough, or other such extreme signs—there should be no attempt at reduction without operation. The risk of the operation is trivial in comparison with that of returning sloughing or ulcerating intestine into the abdominal cavity.

When the coverings of the hernia are so inflamed as to make it probable that sloughing or suppuration has taken place beneath them, reduction should not be attempted without operation; and, even when they are less inflamed, none but very brief and very gentle efforts should be made, for success is improbable, and failure may be mischievous.

The longer the signs of strangulation have existed, the shorter should be the efforts at reduction; and the more acute the signs are or have been, the more gentle should these efforts be. Only, here, do not reckon among the acute signs the intensity of pain in recent or greatly enlarged herniæ; for many of the most intensely painful herniæ are reducible with the help of chloroform, though they may need as much force as is in any case justifiable.

The longer and the more numerous and forcible the efforts at reduction made, in any case, before it comes under your care, the briefer and gentler should your own efforts be; if, indeed, you do not at once decide that enough has been done, and that there remains no fair chance of reduction without operation.

If you find that you have to do with a hernia which has been habitually irreducible, and in which you have reason to believe that, without any addition to them, the contents of the sac have become strangulated, you had better operate at once. You are not likely to reduce a protrusion which even before strangulation was irreducible.

Let me now suppose that, observing these rules, a first examination of a strangulated hernia leads to the decision that its reduction without operation is to be attempted: I cannot give a single rule of practice that shall always suffice for the next step after such a decision; but speaking generally, and of a great majority of cases which come under treatment, it is a safe rule of practice that, after a very warm bath and a few hours' rest in bed—say from three to twelve hours, according to the case—a single attempt at reduction, of reasonable force and length, should be made; that, if this should fail, chloroform or ether should be given; that then, in some cases, but not in all, a second attempt should be made; and that, if this should fail, or if it should not be made, the operation should be performed while the patient is still insensible.

The hot bath should be used in all cases that are not very bad, unless in old and feeble persons, whom it might depress too much. Among these, its place may be supplied by very hot fomentations, or by warm poultices over the hernia and the parts near it; and these must be used as the next best things when a hot bath cannot be had. Helped by rest, all these things are certainly very useful, whether by relieving the tendency to irritable muscular action, or by relieving congestion, or by whatever other means. Especially you may see their utility in hospital patients, who are commonly brought in wretched, chilled, and restless, with their herniæ tense and full and very painful, and their abdominal muscles starting into resistance at the least painful pressure. The heat of the bath, and bed, and recumbent rest, may remedy all this; and the hernia may become easily reducible, or may even reduce itself. It is commonly advised to have the bath so hot, and to keep the patient so long in it, that he may be very faint; and during this faintness to attempt the reduction while the patient is still in the bath. I more than doubt the prudence of this advice. It seems to me better to let the patient be simply soothed and relaxed in the bath, then to put him into bed wrap-



ped in warm blankets, lying on his side, on his back with his knees drawn up, or with his pelvis a little raised, and then, after an hour or two of complete rest, to attempt the reduction. The advantage of this plan is shown in the many cases in which the surgeon gets the credit for reducing a hernia which the house-surgeon has failed to reduce. The house-surgeon tries in the bath, and fails; the surgeon, an hour or two later, succeeds, not, or at least not always, by greater skill, but by reason of the more favourable condition of the patient after a time of rest and warmth, and of his better position—lying flat instead of half sitting as in the bath. This employment of rest and the bath may be helped by opium whenever the hernia is very painful, and the patient too restless to have a chance of natural sleep. A grain of opium may procure the rest necessary for the quietude of the parts, but is less likely to be useful with femoral than with umbilical herniæ, and less likely with these than with inguinal.

In the old, and in others who may have had inactive bowels long before the strangulation, and in whom fæcal accumulations or abundant air may be in the large intestine, an enema even of a large quantity of liquid should be used; for the emptying of the large intestine may greatly facilitate the return of the hernia. Purgatives, I believe, had better not be thought of, if there be any marked signs of strangulation. There are no clear indications for determining the cases in which they might possibly be useful; and, if they do no good, they may do grievous harm. I do not doubt that some have gained advantage from purgatives; but in my notes and memory I rather find instances of mischief, and certainly no cases in which there seemed reason to think that the patients were the worse for not taking any sort of purgatives after evidence of strangulation.

After the warm bath and rest—and still speaking of only the majority of cases, for in some there is no time for these things—you may give chloroform or some other anæsthetic, and try to reduce the hernia. How you are to do this, I cannot tell you now; nor what time and force are reasonable to be used. You must imitate what you see done by men of repute, and use the best common sense you can. I can tell you some things that you must not do. You must not go to work as if you were resolved to reduce the hernia *per fas aut nefas*; you are not to spend an hour or even half an hour about it, or use all your force, or take off your coat and turn up your shirt-sleeves, or kneel on the bed, that you may press with the more weight; you are not to let half-a-dozen persons try their hands in turn. You are not to do these or the like things, all of which I have known as the sources of dire calamities. You are to be gentle and self-restraining, mindful of the delicacy of some of the struc-

tures you are handling, and that you may do them much more harm than would come of the operation which you are trying to avert. These cautions are the more necessary because, when the patient is under chloroform, you have nothing but your own sense and senses to tell you how far you may go without doing harm. The great value of chloroform and ether is that, by abolishing sensation, they put an end to the muscular resistance to reduction which, whether he will or no, the patient makes when hurt by the pressure of his hernia. Hence they are most useful in the herniæ of which the difficulty of reduction is chiefly due to muscular resistance; in the recent, or in the recently much enlarged; in the inguinal more than in the femoral, and in these more than in the umbilical; in the painful more than in the painless. Chloroform and ether are by so much the most potent helps to the reduction of herniæ, that it may seem as if it would be right to use one of them without waiting for the influence of a warm bath, or recumbency, or any similar means. Sometimes it is right thus to do, especially in herniæ that have only very recently come down and are intensely painful. But more commonly, if there would be danger in waiting for three or four hours, it is because strangulation is so far advanced that the operation ought to be done at once, without any previous attempts at reduction. If there be no such extreme urgency for immediate reduction, there can be nothing but advantage in the use of the bath and the three or four hours' rest in bed; for they may make the hernia reducible, or, even if they fail of this, they may cause changes in it which are beneficial for both the performance of the operation and for the probabilities of recovery afterwards.

I have been speaking lately of the plans for average or medium cases; and before, of the cases in which no attempt at reduction without operation should be made. You may ask, Are there any cases in which it is justifiable to wait longer after the warm bath, and rest, and chloroform, and a fair attempt at reduction, have been tried and have failed? I will not venture to say that such a case for waiting cannot happen; but I am clear that your rule, with barely an exception, must be that, when you are satisfied that a hernia is strangulated, and you have failed to reduce it with such helps as I have indicated, you should operate. While the patient is still under chloroform, the operation should be done; and you should prepare for it before giving chloroform. Of course, if you are satisfied that the hernia, though irreducible, is not strangulated, you may wait; but in this case you must watch almost impatiently, for an intestine or omentum that cannot be reduced is very likely soon to become strangulated, and so is one that is inflamed or blocked up in a hernial sac. Still, if no signs of



strangulation, especially if no vomiting, should supervene, you may wait from day to day; but if the signs do occur, especially if there arise vomiting, or increase of pain, or increasing rapidity of pulse and breathing, then you must operate at once, and you had better not try again at reduction. The trial is much more likely to do harm than to do good: you had better operate at once. Nothing does more harm to a strangulated or nearly strangulated hernia than the force of an unsuccessful attempt at reduction.

While you are waiting, you may use, in different cases, ice or warm dressings, enemata, aperients, or opiates. I cannot tell you the indications for each of them. I am glad to have had no sufficient experience of waiting to have weighed the several values of these things; glad, because I have no doubt that, for every case in which an operation has been avoided by waiting, there have been two cases in which lives have been lost by waiting too long. But there are at least one or two conditions favourable for all cases in which you desire to wait; namely, rest in bed and very sparing food. Nothing should be allowed to disturb the patient's rest, and no handling of the hernia should be permitted. Part of the value of ice and poultices and other like applications is this, that they all keep hands off.

Of other supposed helps I will not speak—of tobacco, and curious postures, and shakings with the legs up and the head down, and cupping-glasses, and other like and unlike things. They are ingenious wrong-doings, more dangerous than the operation which they are intended to avert.

In speaking of herniæ as being reducible, I have had in mind only such as can be completely and certainly reduced. But it is not uncommon to meet with cases of strangulated hernia, in which the reduction is doubtful or partial. As I look through my cases, I found many of these recorded, in which there was delay in sending patients to the hospital, because surgeons believed that part of the hernia was put back, and hoped that the rest would soon go; and some in which, even in the hospital, there was mischievous delay through the same fallacious hope. It is not easy to say what takes place in these partial or doubtful reductions. Some patients will tell you that the whole never did go back, and that what now seemed to be reduced might only be an additional protrusion. Sometimes, I think, air is pushed back from the intestine, or fluid from the sac; sometimes omentum is put back; sometimes, perhaps, part of the intestine; sometimes nothing—the whole notion of reduction being fallacious. The liability to deception is greater than you would imagine. You may feel a thrill of receding fluid, or a gurgling of air, which you may suppose to be what some describe as the characteristic gurgling (as if any-

thing of the kind were infallibly characteristic), or something slipping back; but all may be fallacious. There is one practical rule for all these cases. If the symptoms of strangulation be not relieved by the supposed reduction; if the vomiting continue, or the pain, or the patient's sense of distress, or any other of the distinctive symptoms—then, without delay, you must operate. A partial reduction of a strangulated hernia, if it be not followed by a complete relief of symptoms, is in nothing better than no reduction.

I may add, that most doubtful reductions are not reductions; and of the partial reductions, none are safe except some of those in which intestine is put back and only omentum remains in the sac.

Be prepared also for cases in which reduction is, or seems, complete, and yet the signs of strangulation are not relieved. In these, a hernia may be returned *en masse*, or pushed into another sac, or between the peritoneum and fascia; or the case may be one of hernia complicated with an internal strangulation, or one of many other conditions so hard to discriminate and deal with, that I can give only one general rule for their management—viz., that if you can feel a lump at or near the hernial ring, as if there were something which may be a strangulated hernia, then you must operate.

To end what I should say respecting the propriety of operating, I ought perhaps to speak of the condition of the patient as affecting the risk of the operation by reason of age and general health and various complications. Among my cases, I find not only many of the fattest and feeblest, but examples of complication with phthisis, acute and chronic bronchitis, aortic constriction, phlebitis, gastric ulcer, diseased bladder, intestinal disorders of various kinds, and internal strangulation.

Patients such as these where one would not wound for any trivial good; but, with a strangulated hernia, the peril of doing the operation can hardly ever be so great as the peril of leaving it undone. Old age and feebleness, fatness, intemperance, or unsoundness of whatever kind, may add to the risks of this, as of any other operation; but all these risks must be accepted. A patient must not be allowed to die with a strangulated hernia, if by any means whatever, the strangulation can be relieved; and you must not be averted from the operation by any consideration of the number of deaths that follow it. The deaths after the operation may be 50 per cent.; but the deaths due to the operation are not more than 2 or 3 per cent., and even these would probably have been deaths from the hernia had not the operation been performed. The great proportion of deaths is made up of those in whom the strangulation has done mischief which the operation cannot remedy. It is not unfair to main-



tain that, speaking generally, the deaths after operations for hernia are only to be counted as failures to save life, while the recoveries are to be counted as lives saved from certainly impending death.

The design of the operation for hernia is to divide the structures which tightly gird the protruded parts, so that these may be returned. These structures, forming what is called the stricture, are in some cases outside the hernial sac; in some, in its very substance; and, according to these and other differences, the operation may in some cases be completed without opening the sac, and in other cases must include this opening. The advantages of the two methods have been often discussed, and I may begin by speaking of them.

There can be no doubt, I think, that if all the rest of the operation were always the same, the advantage of reducing the hernia without opening the sac should always be sought. Thus to reduce a hernia is the next best thing to reducing one without any operation at all. The structures divided externally to the sac are insignificant, and it might be difficult to name an operation less endangering either life or health than this would be. The peritoneum is not wounded; the intestine and omentum are not touched or exposed to air; the wound may be small; any hemorrhage may be easily stayed, and must be all external. Thus the wound is favourable for speedy healing, and erysipelas or any other mischief is not likely to extend to the peritoneum.

These are sufficient reasons for always wishing and generally intending to operate without opening the sac, especially in old and feeble people and in cases of large herniæ. But you must not let your wishes carry you too far. They may lead you into great mischief. For, first, there are many cases in which the contents of the sac are not fit to be returned into the abdomen—for instance, when they are sloughing, or deeply ulcerated, or strangulated within the sac. The risk of returning these is so much greater than that of opening the sac, that you should not hesitate to open it whenever you have any, even slight, reason to suspect any of these conditions of the sac's contents. And such suspicion there must always be in these sets of cases—1, in those in which the strangulation has existed long, say four or more days, whether with slight or with severe symptoms; 2, those, of whatever date, in which the signs of strangulation are very acute; 3, those in which there are very marked signs of advanced or low inflammation in and about the sac; 4, those in which the contents do not go back easily and within two or three minutes after all stricture is fairly relieved. And, in measuring this case of going back, you must be scrupulous; for I have certainly done harm, and seen more done, by trying

too long to reduce herniæ without opening the sac—fingering and clearing the parts, and pressing them many times; damaging intestine, and exciting suppurative inflammation all about the sac; so that when, at last, the operation was completed, much more mischief had been done than if the sac had been at first straightforwardly opened.

I think, then, you may take this as a safe rule in all ordinary cases; intend to complete the operation without opening the sac, but give up your intention if you find any reason to suspect complications, or very morbid states of the parts within the sac, or any such difficulties as would lead to the use of forcible or long continued efforts at reduction. Of course, the more you practise the operation, the less frequently will these difficulties hinder you; but you may keep to the same rule, and think it wiser to avoid difficulties than at all cost to overcome them. And, finally, when you are in doubt, open the sac; for, though the advantages are, on the whole, clearly in favour of not opening the sac, yet the amount of advantage is not so great as to justify any considerable risk for it. Rough statistics of operations are not to be taken as measures of this advantage. In the worst cases, the sac must be opened; and, of these, a large portion die without any regard to the manner of the operation.

As to the method of operating, there are many, and some good, anatomical rules, on which I shall not touch. They are admirably laid down in Lawrence on *Hernia*. I will only give some general rules, such as may be useful in nearly all cases.

In all, you should decide at first, if possible, whereabouts the stricture is, so that your first incision may be fairly over it, and give you room to act on it without needless length of cutting. In femoral hernia, you may be sure that the stricture is at, or within half an inch of, the femoral ring; and this is near enough for guidance for the first incision. So, in umbilical hernia, the mouth of the sac is always the seat of the stricture; and the middle of your first incision may be right over it. In inguinal hernia, the stricture is, in the large majority of cases, at or within the internal inguinal ring; and the incision should extend from the internal ring to beyond the external ring, and (according to the characters of the hernia) to a greater or less distance towards or along the scrotum. But, in inguinal hernia, the stricture may be at the external ring, or, being formed by the thickened mouth of the sac, may be pushed up towards the abdominal cavity, or down along the canal, or beyond the external ring to some distance in the scrotum. Moreover, in some rare cases of congenital hernia, there may be two strictures—one at each end of the open canalis vaginalis. For these cases, it is useful to apply a rule devised, I think, by Mr. Luke, for ascertaining the seat of stricture; namely, to observe at what point



along the course of the hernia the impulse on coughing ceases. For, when a hernia is strangulated, the impulse can be felt as far as the stricture; beyond that, it cannot be felt: therefore, where the impulse ceases, there probably is the stricture; and this part must be fairly included within the length of your incision.

In femoral hernia, your first incision may be vertical, in a line drawn straight down from the spine of the pubes—a projection which you can always easily feel. This incision seldom needs to be more than an inch and a half long, and may sometimes be less. In umbilical hernia, a vertical incision of an inch and a half or two inches will suffice, in the middle line, so as to reach either the upper or the lower border of the mouth of the sac. But I am disposed to believe, though I have not tried it, that in large umbilical herniæ, two incisions, going to opposite borders of the ring, would be better than any one. In inguinal hernia, the incision should take the direction of the neck and upper part of the hernia, and its length must vary according to the size of the parts to be returned.

Through these incisions you must go on and on, through the several layers which your anatomical dissections of healthy parts will sometimes enable you to recognise, till you come to the sac. The thickness of these layers is more various than you may suppose. Especially in small femoral herniæ, you will often find an unexpected quantity of fat about the sac; and in umbilical herniæ, very much more fat about the mouth of the sac than the thinness of the integument over it would at first suggest.

Through whatever thickness, you must continue cutting in the same direction; and when you are fairly on the surface of the sac, keep to the same line. Do not clean the whole front of the surface of the sac; do not separate it from the surrounding textures. No good can come of this, but much harm may. You want nothing more than a linear division of the stricture, whether with or without a linear opening of the sac. All that is done on either side of this line is useless or mischievous.

When you have thus fairly reached the sac, and have exposed its neck and mouth or narrowest part, you must proceed differently, accordingly as you propose to complete the operation without or with an opening of the sac. In the latter case, you open the sac first near its mouth, and then along the length and full extent of your external incision; and then divide the stricture from within. You may do this on your finger-nail or a director, and with cares about the intestine and other structures, which are insisted on in all handbooks. In the former case, when you intend not to open the sac, you must find where the stricture is, and divide it outside the sac.

Among femoral herniæ, there are differences as to the seat of stricture which I cannot explain to you, but which my cases made very clear to me. In some instances, as you trace up the neck of the sac, you find it tightly banded across by the layer of fibrous tissue called Hey's ligament—a layer traceable as a falciform edge of the fascia lata, where that fascia is connected with the crural arch, and is thence continued to Gimbernat's ligament, bounding the upper part of the saphenous opening.

Sometimes a fair division of this layer of fibres up to the edge of the crural arch is sufficient to render the hernia reducible; and here, with the reduction, should end the operation. But, with more cases, this is not sufficient; and you may feel the stricture formed by bands of fibres which encircle the neck of the sac, and which must be divided, band by band and layer by layer, till none can be felt. These fibres are, I believe, bands of tough connective tissue, belonging to the cribriform fascia, through which the hernia has protruded. Very rarely, however, even the division of these, and of Hey's ligament too, is not sufficient; for the stricture is formed by thickening of the mouth of the sac itself. This condition, which is the common cause of stricture in inguinal hernia, is very rare in femoral; but it certainly does occur; and, in any case well suited for the operation without opening the sac, you may try to thin the mouth of the sac without opening it, and thus to make it extensible enough for the return of its contents. You may try this; but let me add, the chances of success are small. You are much more likely to cut into the sac at some thin place; and, when you have done this, you may as well enlarge the opening and divide the stricture from within.

It is this uncertainty as to the exact cause of stricture in strangulated femoral hernia, which makes it advisable to put on a strong resolve not to use too much time or force in endeavouring to operate without opening the sac. You will be apt to think, when you have divided one thing, that now you will be successful; and you try to press back the bowel. But you fail; and then you divide something else, and try again; and now again you fail. And thus you may go on, till you have done more harm than you would have done by a straightforward opening of the sac and immediate easy division of its stricture and return of its contents. Keep this in mind, that an easy reduction from an open sac is better than a difficult reduction from a closed one.

In umbilical hernia, the case is simpler. The stricture is always in the tough fibrous tissue of the sheath of the rectus. When you have reached this, commonly going much deeper through fat than you would expect, and not only deeper, but further under the sac, you may be able to divide the stricture



without opening the sac; but this is difficult, for the sac is always very thin, and there may be little tissue between it and the fibrous ring. Still it is well to try, but not too long. You are more likely to open the sac at or close by the stricture; and, if you do this at all, you may as well save time and force by opening it more widely.

In inguinal hernia, the stricture is in many cases, and was in the majority of those on which I operated, formed by the mouth of the sac, thickened and hardened by what appears to have been an inflammatory process, producing a scar-like and contracting tissue. This tissue forms a band about a fourth or a third of an inch in width, and about a line in thickness, and sometimes has a thin sharp inner edge. By the way, let me say that the formation of this band is not the result of wearing trusses. I have seen it very marked in patients who never wore a truss; and I have seen the sac thin and soft in every part in those by whom trusses have been long worn. But, however it may be formed, this annular thickening and contraction of the mouth of an inguinal hernial sac is a common cause of stricture. On account of it, you should proceed at once to expose the mouth of the sac; unless, indeed, you should have found the tissues outside it so tight that you may fairly believe the division of them will be sufficient for permitting the reduction of the hernia. When you have exposed the outside of the thickened mouth of the sac, you may still achieve the reduction without opening, by gradually thinning the mouth—dissecting off band after band from any portion of it. Sometimes the mouth will yield sufficiently at a part thus thinned to stretch and allow the reduction; but more often, I think, you will fail, and will have to open the sac and divide the stricture from within.

Here, as with femoral hernia, be scrupulous not to spend more than fair force and time for the sake of the reduction without opening the sac. It is a good thing to succeed; a bad thing to fail. You must measure how much risk of harm it is right to incur for the hope of doing good.

If the reduction of hernia be accomplished without opening the sac, you will have attained the best immediate object of the operation; but remember that fallacies of reduction are possible here as well as in the cases in which no operation has been done; they are, however, less mischievous, for, if the stricture be completely divided, there will be no strangulation of whatever remains in the sac. Especially you must have no fear if, as commonly happens, after returning intestine, some omentum remain in the sac. This will do no harm; but if more than omentum have remained in the sac, and the signs of strangulation be not relieved or lessened, you must operate again and

open the sac, regarding these cases in the same light as those of partial or doubtful reduction, of which I spoke in the last lecture. But suppose the sac opened, as it should be in nearly all bad cases, and in many which, though they are not bad, yet may be called difficult, here may occur the most difficult question of all, What is to be done with the contents of the sac? Of course, in most cases you are to return them; but in many you are not, and which are which?

Look first to the character of the fluid which, in most cases, you will let out of the sac. In most cases, not in all; for, in some small femoral herniæ, especially in very thin dry people, and in many umbilical herniæ, and in any that contain a large quantity of omentum, there may be no fluid, or too little to be distinctly seen; but if there be enough to judge from, you may deem it a good sign if the fluid is clear, and yellowish like serum, or, rather, like liquor sanguinis—for it will coagulate spontaneously. This indicates only such an exudation of fluid as may come from a simply congested piece of intestine, or from a piece not badly inflamed; and the cases would be very rare, if there can be any, in which intestine found behind fluid such as this might not be returned. The same may be said when with fluid such as this there are flakes or bands of lymph or fibrinous exudation; for these tell of only such inflammation as may safely be recovered from when the intestine is returned. I am disposed to say the same of the cases in which the fluid is clear, but more or less deeply blood-stained; for this exudation of blood-cells or blood-colour is not characteristic of any serious morbid change in either the sac or its contents. But when the fluid of the sac is turbid, brownish, muddy, it tells of more advanced changes in the intestine or in the omentum; and the further it goes in this direction the more carefully must you consider whether these are in a fit state to be returned. You will probably have to decide that they are not fit, when the fluid has a distinct fæcal or putrid odour; and of course they are not fit when the fluid has fæcal matter mixed with it.

I do not venture to say that the characters of the fluid contents of the sacs of strangulated herniæ are to be absolutely relied on as guides for practice; but they are good evidence to be taken into the general account, for they fairly represent the state of mere congestion or inflammation, or more or less advanced decay or decomposition, or giving way of the walls of the strangulated intestine and omentum.

Not rarely, when you have divided the stricture and returned the contents of the sac, fluid runs from the peritoneal cavity. I do not know any rule of practice but that you must let it run as long as it will, and, if it be of very unsound appearance, not close the wound till the fluid has ceased to flow, if even then.



As to the omentum which the sac may contain, and what to do with it; if there be a small quantity—say two or three square inches—and this be not adherent, and not more changed than by congestion or slight inflammation, there can be no question that you are to return it after the intestine; and if there be a piece of even very large size, and not more changed in texture, you had better return it if you can without much force or expense of time. But it sometimes happens, when the abdomen is tense with over-filled intestine, that you cannot return a large piece of omentum without much difficulty. What then? shall you cut it off or leave it in the sac? I advise you to leave it. I believe that the cutting off, with the necessary ligatures or other fastenings of vessels, adds to the dangers to life; while the leaving of omentum is only sometimes followed by greater difficulty in the fitting of a truss—a difficulty which is not great enough to justify any risk of life.

Still more may this rule of leaving omentum in the sac be observed when a large piece of it is hardened and thickened as by old disease. When a small piece is thus changed you may, I believe, return it.

When omentum is adherent to the sac, but in other respects fit to be returned, you should break the adhesions and return it, after stopping all bleeding. If it be not fit to be returned, leave the adhesions; and in any case do not break adhesions so near the mouth of the sac that their vessels are likely to bleed into the abdominal cavity.

When omentum is sloughing, or nearly sloughing, leave it, that it may cast its sloughs outward.

But the chief questions in these operations are concerned with the state of the strangulated intestine and the manner of dealing with it. You are to judge chiefly from the colour and the tenacity. Use your eyes and your fingers; sometimes your nose; very seldom your ears, for what you may be told about time of strangulation, sensations, and the rest, is as likely to mislead you as to guide aright.

As to colour, any tint, from the natural grey through various shades of rosy or ruddy pink, or redness, up to the deepest crimson, even verging to blackness, may be consistent with fitness for returning of the intestine, if the texture be good. All these tints may be due to congestion and stagnation of blood, or to extravasion of blood into the intestinal walls; and all these may have been without such inflammation as would spoil the texture of the intestine, and may not have endured long enough to kill it. I am disposed to say that you may return intestine of any colour short of black, if its texture be good; if it feel tense, elastic, well filled out, and resilient, and collapsed or sticky; and the more the surface of the intestine shines and glistens, the more sure you may be of this rule.

When a piece of intestine is thoroughly black, I believe you had better not return it, unless you can be sure that the blackness is wholly from extravasated blood. It may not yet be dead, but it is not likely to recover; and, even if it should not die after being returned, there will be the great risk of its remaining unfit to propel its contents, and helping to bring on death by what appears very frequent—distension and paralysis of the canal above it. But, indeed, after blackness of strangulated intestine commonly tells of gangrene already; and of this you may be sure if the black textures are lustreless, soft, flaccid, or viscid, sticking to the fingers or looking villous. Intestine in this state should never be returned.

Colours about which there can be as little doubt, for signs of gangrene, are white, grey, and green, all dull, lustreless, in blotches or complete over the whole protruded intestine. I cannot tell why there should be so many colours in different cases, or sometimes even in the same case; but all are alike certain signs of gangrene, and they are always combined with loss of due tone and texture of the intestinal wall. Intestine with these marks, even though they be small, must not be returned.

Then, as to the texture of the protruded intestine: it should be, for safety of return, thin-walled, firm, tense, and elastic, preserving its cylindrical form, smooth, slippery, and glossy. The further the intestine deviates from these characters, the more it loses its gloss and looks villous; the more it feels sticky and is collapsed and out of the cylinder form, the softer and more yielding, the more pulpy, or like wet leather or soaked paper, the less it is fit for return. And when these characters are combined with so many colours as I have described, the intestine must be taken to have perished, and had better be laid open, that its contents may escape externally and do no harm.

But short of gangrene, there may be ulceration of the walls of the intestine. The usual place for this is where the intestine is girt by the mouth of the sac, and it is most frequent in femoral herniæ long strangulated. In these it is especially the sharp hard edge of Gimbernat's ligament, which seems to cut into the intestine, thinning its wall and at last piercing it; and the chance of this having happened is enough to justify the rule that, where the strangulation has been sharp and long, the intestine should be gently drawn down after the stricture is divided, in order to see that there is no great injury of its walls where the chief pressure of the stricture has fallen on them.

Here, too, because they are similarly dangerous to life, I might speak of laceration of the intestine in too violent attempts at reduction, or wound of it in operation; but I have



no personal experience of such cases, and can add nothing to what you may read in the best treatises on hernia—such as that of Sir William Lawrence, or in the chapters devoted to hernia, by Mr. Erichsen, in his *Science and Art of Surgery*, or by Mr. Birkett in Holmes's *System of Surgery*. These will supply you not only with their author's experience, but with what they have gathered and set in order from the writings of others. Limiting myself to what I have studied in my own cases, I must omit many things besides ruptured and wounded intestine; such as the various complications of strangulated hernia with hydrocele and misplaced testicle, with varicocele, and with accidents of the operation, such as hemorrhage from the epigastric or the obturator artery. Some of these things I have never seen; others I have seen only once or twice, and have learnt concerning them nothing but what you may learn by reading the works to which I have referred to.

But, as to the treatment of sloughing and ulcerated intestine of which I was just speaking, I will only say that it has always seemed to me more prudent to incur a great risk of having a permanent external faecal discharge by leaving the intestine at the wound, than to add to the risk of life by returning any thing which it may seem possible to repair by suture or any such means. Of course, these means are not to be thought of if the sloughing or ulceration be of more than very small extent; but even in the smallest, unless in some very rare cases, I would not add to the inevitable risks of life by returning the damaged intestine. In cases of hernia, the saving of life is so much more important than anything else, that we ought not to incur a risk of life for anything less than the highest probability of saving a patient from some life-long distress.

After the action of the bowels, if all still go on well, there is no need of considering anything but the patient's comfort, and the local treatment, and the diet may be of the plainest kind. Excess is more likely to be mischievous than spareness.

Among cases that do not go on well, there is a great variety; but I shall speak of those alone in which the wrong is in something peculiar to hernia. Of course the wound of the operation may lead to any of the evils that may follow other wounds, as erysipelas, pyæmia, and the rest; but these I shall almost pass by.

Among my cases, I find some in which ill-looking symptoms, such as diarrhoea or colic, followed the operation, but meant no serious mischief, being only the continuation of the intestinal disturbance which preceded the strangulation. I have already spoken of this matter, but it is worth repeating: that it is common for the descent and strangulation of a hernia to be preceded by some intestinal disturbance, the signs of which may

be suspended during the strangulation, and renewed after the operation. Whatever the disturbance, it may be treated, or left alone, as if no operation had been done; but it must not be forgotten in estimating the meaning of any symptom of ill-doing.

Of these symptoms, one of the chief is vomiting. If a patient vomit once or twice soon after an operation for hernia, it may only be because the stomach was filled with secretions before the operation, and now finally empties itself. In this is no harm—perhaps some good. But, if the vomiting continue, it may be a sign of the gravest import. It may, indeed, be due to chloroform. I have seen several instances of this, and among them one in which chloroform-sickness continued for three days after the operation, endangering the patient's life. Generally you may distinguish the vomiting due to chloroform from that due to peritonitis or unrelieved strangulation, by its being attended with horrible nausea, like sea-sickness; by its producing no fæcal fluid or abundant gastric secretion, there being more retching than vomiting; and by the patient's feeling relieved of his hernia, however wretched he may feel with his nausea. If the pain and distress of the hernia be relieved by the operation, and, except for vomiting, all seem well, you may be nearly sure that the vomiting is due to chloroform, and commonly you must wait till it spontaneously subsides. I believe you cannot cure it, and food put into the stomach only aggravates it. The patient had better be without food, if in fair strength; but, if very feeble, he had better be maintained with enemata of milk, eggs, beef-tea, and wine.

If vomiting go on for six or more hours, or for days, after the operation, and be not due to chloroform, it is a very bad sign—I had nearly said a mortal one; for generally it tells that the operation has failed in its design. Either the strangulation is not reduced, or the intestine is paralysed above the strangulation, or there is peritonitis or sloughing or perforation of the intestine, or some such trouble. Very rarely the untoward conditions indicated by continuous vomiting are relieved spontaneously or with repeated doses of opium.

The persistence of abdominal distress, with tension and pain and colic, after the operation, is not so serious. If other symptoms be relieved, these probably will be; and they may generally be treated with large enemata or aperients, such as the sulphate and carbonate of magnesia. But there is no need of haste to give these things. The abdominal troubles which they are to relieve are not due to serious disease, but probably to intestinal accumulation, which began before the strangulation, which would be very slow to destroy life, and may be allowed to remain till it can be treated without risk. In such a case as



this—a not very rare case—when all seems well except the abdominal pain and tension, be on the watch. If there be no change, do nothing. With time, the intestines will empty themselves. If there be increase of pain, without increase of pulse or breathing or other evidences of fever or inflammation, give enemata or aperients. I am sure you may manage cases of this kind better and more deliberately than I did some of those which I have recorded. I see now that I was over-busy with them, and was only very fortunate in that I did no harm.

A large group of cases, much worse than these, may be made of those in which the operation gives little or no relief: all goes on after it as all did before, or everything becomes worse. Few cases can be more grave than these. You may think yourselves happy if, from twenty such cases, you can save one.

The failure of relief from the operation may be due to the utter exhaustion of the patient. I have had to operate on patients already dying. I could not refuse to operate, for I could not be certain that it would be useless; but it proved useless, and seemed mischievous. The intestine was returned, and all put right; but the patient was too exhausted, as one may say, to be conscious of relief, and went on dying, although carefully fed and nursed.

Or the failure of relief may be because the strangulation remains—a constricting band or misplacement of the intestine, or some such thing; having been overlooked. In such a case, especially if you have not opened the sac, you must open the wound and the sac; enlarge the opening through the stricture; find, if you can, the strangulated intestine or whatever may be wrong, and, if possible, set it right. You may be so fortunate as to succeed—more fortunate than I have been. But then, in two of my cases, an internal and distant strangulation coincided with that of the hernia. This was irremediable—could not be reached; and the second operation was as useless as the first.

But by far the most frequent cases in which the operation gives no relief, or only some slight and very brief relief, from the signs of strangulation, are those in which the intestine does not recover itself. It remains nipped, powerless, congested or inflamed, spoiled, and incapable of contraction; while the part of the canal above it becomes distended, and after a time powerless, the abdomen becoming constantly more tense, though it may be with less turbulence of the intestines. This condition is most frequent in the old, and after long strangulation; it is commonly attended with peritonitis; and no doubt the inflamed state of the muscular tissue of the intestine, both in and above the strangulated part, is often the cause of the loss of muscular power, or a serious hindrance to its recovery. But peritonitis is not a necessary part of the calamity. I believe I have had cases

in which no peritonitis existed beyond the strangulated part. In such cases as these, you may find the best hope, though the best is seldom good, in opium, food, and wine, all of which you must give by the rectum, if the stomach will not retain them. Your object must be simply to keep the patient alive while, in time, the intestinal canal, being at rest, may regain power. Very small quantities of food may suffice for this—a few spoonfuls by the mouth every hour or two; and, twice or three times a day, injections into the rectum of milk, beef-tea, egg, wine, and laudanum, mixed all together.

Very similar to these cases are those in which peritonitis, having begun before the operation, is not relieved by it. There is, plainly, no reason why the relief of strangulation should at once remedy the peritonitis which the strangulation produced; and it sometimes seems to continue quite unaffected. Indeed, you had better expect that this will be so, and follow what is a very good general rule—that, namely, of giving opium directly after the operation in all bad cases of strangulated hernia, unless there be some clear reason for not giving it. I have seen no harm come from this plan, and I think it has sometimes done real good. What are the bad cases I have already told you. In any or all of them, you may give a grain of opium, or an equivalent subcutaneous injection of morphia, directly after the operation; and then watch and determine whether to give more, or what else to do. Especially, you may expect to have to give wine very soon after the operation, for the patients are generally feeble.

Thus, then, you may have in mind four sets of cases in which the operation for strangulated hernia, though, to all appearance, well done, does no good at all, or too little to be counted. It may be so in patients utterly exhausted; in cases of persistent strangulation at the hernia or elsewhere; of intestine rendered powerless; of continuous peritonitis. Of all these I have seen examples. There may be others which I have not seen or have overlooked. From all of these you may distinguish in thought, and generally in fact and practice, those sets of cases in which untoward events arise after a clear interval of relief from the operation. All may go on well, or at least not badly, for a time; the patient may have a clear sense of relief; and the sickness and other bad symptoms may cease for hours or some few days; and then come trouble and disappointment.

The cases that may be thus grouped are many and various. I find among my notes instances of acute inflammation of the hernial sac and its coverings; simple acute peritonitis, such as might be called traumatic; asthenic peritonitis, probably of erysipelatous nature; acute peritonitis, with rapid collapse, from intestinal perforation or rupture; simple inaction or



paralysis of intestine. And to those which were peculiar to operations for hernia may be added other calamities, such as might occur after any operation, as diffuse cellulitis, erysipelas, phlebitis, and others.

It is not possible to speak of all these things now—hardly even appropriate; for, though those in the first list are associated with hernia, they are not peculiar to it, and their pathology and treatment must be studied on a broader field. The peritonitis must be dealt with according to the same rules as that which has nothing to do with hernia, using great care to determine which of the forms I have enumerated is in each case present. The strangulation may itself cause peritonitis; and this, as I have said, beginning before the operation, may continue after it. The wounding and exposure of the peritoneum may excite its inflammation, or greatly aggravate that which existed before the operation. In both these sets of cases, the signs of peritonitis will be observable directly after the operation, or, at the furthest, within twelve hours after it; and these will generally be cases of acute, or, so far as the constitution of the patient will allow, of asthenic inflammation. But you may generally distinguish from these the cases in which peritonitis sets in after a longer interval, and in which it is rather of an asthenic type, whether it come from giving way of the intestine, or from such conditions as would produce erysipelas after external injuries.

Do not suppose me to pretend that, in every case of peritonitis after hernia, you can easily determine to which of these different types of inflammation it belongs. You can determine often; you should try to do so always; for according to the type of inflammation must be your treatment. In the sthenic inflammations, you may give great comfort and help to recovery by free local bleeding with leeches, by large poultices over the abdomen, by weak saline and alkaline drinks, by the plainest and least stimulating diet. In the asthenic, opium is the only remedy that is generally useful. It used to be calomel and opium; but I believe the calomel did harm oftener than good. And with the opium must be rest and warmth, and liquid nutriment and diluted wine. And I can state no more general rules than these; for the rest of the treatment must be determined separately for each case.

Now, besides these very serious evils that may follow operations for hernia, some local troubles may ensue, of which a few are worth telling you of.

Perhaps the most common is acute inflammation of the hernial sac, alone or with the immediately adjacent part of the peritoneum. It is, I think, most likely to happen after operations for large herniæ in old people, when considerable force or

time has been used in the reduction. You may know it best by the localised pain and tenderness with acute fever, without the signs of unrelief or of general peritonitis of which I lately spoke. There is no severe illness, but the general condition appropriate to a sharp local traumatic inflammation; and you may treat it, in this view, with complete rest and warm moist coverings of the part, and very little food; and in the acute cases in robust people, with copious leeching. In the acutest case which I have seen, I applied ninety-eight leeches in three days after the operation, with great comfort, and I think great advantage, to the patient. That was twenty years ago; but it would still be right to do the same.

Another local trouble is acute inflammation of the cellular tissue outside the sac. It is chiefly seen after operations for scrotal hernia and for deep-seated femoral herniæ. Commonly, the case appears going on well for a few days; and then comes a blaze of inflammation in and beneath the skin, leading sometimes to abscess, sometimes to diffuse suppuration. There is in these cases nothing peculiar to hernia. The same troubles may, as you know, follow any other operation; and they always need the same general means of treatment. I have never seen serious evil ensue in any case of the kind.

I might tell of other hindrances to recovery from the operation for hernia; but I limit myself to those of inflammation of the testicle, of sloughing of the scrotum, and others which I have been able to study practically, and I pass the others by, as I have done many other things, especially the complications of herniæ with various local diseases, such as hydrocele, varicocele, undescended testicle, and others. I have seen only one or two cases of each, and can tell nothing which is not already well told about them. Indeed, for a conclusion, I must say that, though to some of you it may have seemed excessive to give four lectures on strangulated hernia, I have treated the subject very superficially, very incompletely. One lifetime is not nearly enough for its complete personal study, in even so large a field as this hospital supplies.—*British Medical Journal*, April 6 and 27, May 18, June 1, and July 6, 1872, pp. 359, 437, 515, 573, 6.

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#### 46.—THE TREATMENT OF STRANGULATED HERNIA BY PUNCTURE OF THE INTESTINE.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

The very great importance to surgical practice which the introduction of any plan for the treatment of strangulated hernia implies, induces us to refer to a method suggested by



Mr. Bryant, in a clinical lecture delivered at Guy's Hospital, in the month of February of this year.

It will be in the recollection of some of our readers that, in the third lecture of his series (which appeared in our columns on April 20), Mr. Bryant gave the details of a case which occurred to him in 1871. It was that of an old man, aged 71, the subject of strangulated scrotal hernia, upon whom he had performed herniotomy, it being necessary to expose the bowel in order to return it. Owing to the restlessness of the patient, however, the gut again descended in a few hours, and all attempts to reduce it failed, until the large intestine was punctured in four or five places with a grooved needle, and a quantity of flatus (but no fæcal matter) let out. The patient recovered without a single bad symptom. In his subsequent remarks upon this case, Mr. Bryant observed that it proves clearly that puncturing the intestine freely may be resorted to with every prospect of success in some cases of hernia. He raised the question whether it is not possible to reduce some herniæ without any operation at all, and he stated that he had made up his mind to try the practice in large scrotal and umbilical herniæ, and thought favourably of its employment in all cases of acute internal strangulation due possibly to bands or twists.

Since the publication of this lecture the treatment of puncturing the bowel to remove strangulation has been followed in three cases (which are now well known), two of which have been published. The third occurred in the practice of Mr. Hulke, in the Middlesex Hospital, in the early part of May. The case was one of internal strangulation from a band, in which abdominal section was performed, and the distended small intestine punctured (ultimately laid open) before reduction of the bulging portions could be effected and the wound closed. This case, with Mr. Hulke's remarks, will shortly appear in our pages.

Of the two published cases, one was reported by Dr. Léon Labbé, of the Hôpital La Pitié. It was a strangulated inguinal hernia, of about eighteen hours' duration, in an old man, aged 70. Here, as the taxis failed, puncture with the aspirator was proposed, and a No. 2 needle introduced. In eight days the patient was well. The other case was one brought before the Académie de Médecine, on May 21, by M. Demarquay, a brief notice of which was given in the *Medical Times and Gazette* of June 1. In this instance, after taxis had failed, a fine trocar was passed into the centre of the tumour, which was a strangulated congenital inguinal hernia, and by means of Potain's aspirator about 120 grammes of intestinal liquid was drawn off into the recipient. No ill-consequence followed, and the patient rapidly got well.

Thus, then, this new mode of treating certain kinds of hernia, which has been tried and suggested by Mr. Bryant, has since found favour in English and foreign practice; and we hope, as time goes by, to be able to report favourably of it as a means of relief in those cases in which hitherto little or no success has crowned surgical efforts.—*Medical Times and Gazette*, Aug. 3, 1872, p. 120.

#### 47.—STRANGULATED INGUINAL HERNIA OF THE RIGHT SIDE.

ASPIRATING PUNCTURE; ISSUE OF FLUID AND GAS; IMMEDIATE REDUCTION; CURE.

By Dr. LEON LABBE, Surgeon to the Hôpital La Pitié, Vice-Professor at the Paris School of Medicine.

The extreme importance which the process of reducing hernia after an aspirating puncture must rapidly assume in surgical practice, the limited number of facts which have been known until now, and the interest there is in encouraging medical men to follow this direction, induce me to communicate the following case which I have just had an opportunity of observing.

In the night of June 20th Mr. D., aged seventy, an exceptionally strong and robust individual, after a violent fit of coughing felt an intense pain in the right inguinal region. This was followed in a few minutes by nausea and vomiting; whilst a somewhat large tumour showed itself in the right inguinal region.

On the 21st, at 6 p.m., I first saw the patient, together with his ordinary medical attendant, who had employed taxis with great care, but to no effect. I then tried taxis myself, but was equally unsuccessful. The nausea and vomiting still continued; pulse 75. Taking into account the circumstance that strangulation dated only about eighteen hours, and that probably the anatomical lesions would be very slightly advanced, I without hesitation proposed puncture with the aspirator, and without further delay I introduced the No. 2 needle. About ten grammes (two drachms and a half) of a yellowish liquid immediately escaped, together with a quantity of gas which I cannot exactly estimate. The tumour, which was as large as the fist, flattened immediately; and a very gentle pressure exerted for one minute near the neck of the sac caused complete reduction of the hernia. The patient felt immediate relief, and expressed his satisfaction.

During the few hours which followed—from 6 to 11, and more especially between 8 and 11 p.m.—there was a little vomiting, and more particularly nausea. The patient was somewhat feverish, and had slight rigor. I had administered,



as I always do after reduction of hernia, whether by taxis or operation, pills of the gummy extract of opium (each containing one centigramme—one-sixth of a grain,—to be taken one every two hours, so that ten or twelve centigrammes may be absorbed in twenty-four hours), with the object of bringing on paralysis of the intestines. At 11 p.m. the symptoms above alluded to all disappeared, and from that time the patient enjoyed the most complete comfort.

June 22nd. Pulse 60; countenance normal; scarcely any tenderness in the abdomen over the inguinal region.

23rd. Three natural stools; no pain or fever; appetite excellent.

Eight days afterwards the cure, which had been evident even on the third day, had become quite permanent. The patient's health is now excellent.—*Lancet*, July 20, 1872, p. 78.

#### 48.—ON THE TREATMENT OF HEMORRHOIDS.

By the EDITOR OF THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

[The following is from a review of works on the Surgery of the Rectum recently published by the following authors, viz.: Dr. Wm. Bodenhamer, Dr. W. H. Van Buren, of New York; and Mr. T. J. Ashton, Mr. H. Smith, and Mr. Wm. Allingham, London]

In the treatment of hemorrhoids we have to deal, firstly, with the thickened and inflamed portion of the mucous membrane of the rectum and with the enclosed venous plexus; secondly, with the large arterial supply from Quain's parallel arteries from above; thirdly, with the muco-cutaneous membrane of the anus liberally endowed by branches of the pudic nerve, continuous below with the skin of the perineum. What line of action can be clearer; we must remove the excrescence, prevent hemorrhage, and obviate nervous irritation and preventible pain; and avoid risk of phlebitis and of pyæmia.

Dr. VAN BUREN says: "The means which have been employed to destroy the tumours are various; I have tried them all except excision, and can confidently recommend to you *strangulation by the ligature* as the safest, surest, and most manageable procedure.

"The use of the knife or scissors was fully demonstrated by Dupuytren's experience to be dangerous; he lost several cases from hemorrhage, which comes on insidiously after the operation—the blood not escaping externally, but accumulating gradually in the cavity of the bowel. The actual cautery is a repulsive procedure, and not easily applicable under all circum-

stances. Galvano-cautery promises well, when proper apparatus is at hand. Of the potential caustics, nitric acid acts too slowly; and the others, in addition to this objection, are unmanageable. Chassaignac's *écraseur*, and its modifications, in which iron or copper-wire is substituted for the chain, require more time in their application, and bleeding *does* sometimes follow their use in this operation. The various clamps recommended by the instrument makers are, to say the least of them, unnecessary; and injection of the tumours with solution of persulphate of iron is painful and inefficient. On the other hand, a stout ligature of silk, or gut, or hempen thread, is always readily obtained; its application requires no great amount of anatomical or surgical skill; and the result you will find certain and satisfactory—if you follow the rules I am about to give you.

“The patient being in good condition for the operation, with bowels acting regularly and well, let him delay his daily stool until your visit, and present himself to you immediately afterward, with his piles thoroughly protruded; let him stand, bending forward over a bed or chair, with the parts exposed to a good light. Having provided yourself with a tenaculum, a double hook—such as is found in every operating-case, forceps, scissors, and several stout needles armed with long double ligatures, seize the largest of the tumours with your hook—which you transfer to an assistant, telling him to draw gently upon it; then pass a curved needle pretty deeply through the base of the tumour, draw it through to the middle of the double ligature, cut the needle free, and proceed to tie one of the ligatures as deeply as possible, at either side of the base of the tumour, drawing your first knot tightly, so as to strangulate the included tissues thoroughly. Repeat this procedure upon each of the remaining tumours—there are rarely more than three or four at the most, sometimes only one or two—cut off your ligatures short, and then carefully replace the strangulated tumours within the cavity of the bowel. This is the outline of the operation; now for the details. If your patient cannot get his bowels to act at the time of your visit, or if the tumours do not come down satisfactorily, let him have an enema of tepid water, and try again. If they tend to retract during the operation, let him sit over warm water and strain; and it is well to have a curved spatula, or Sims's speculum at hand; also, to transfix and thus secure all the tumours you propose to ligate, before you begin to tie. Introduce your curved needle from without inward, protecting the gut from its point by your finger; strive to get well up into the bowel, and if possible, avoid including any of the delicate semi-mucous integument of the anus in your ligatures, as this increases



greatly the pain of the operation at the moment, and afterwards. If you are successful in this, the pain of the operation is really trifling. If you cannot succeed to your satisfaction, it is better to divide the integument on the anal side of the tumours' base by the knife or scissors, and, in tying, lodge your ligature in the groove thus made. This is a practical point of importance, for the delicate semi-mucous membrane of that portion of the rectum habitually grasped by the sphincter is far more sensitive to violence than the gut within; and, when included in a ligature, it is painfully pinched by the irritated muscle, becomes cedematous and rolls out at the anus, giving the patient the unpleasant idea that his piles have come down again. Moreover, like one of the varieties of external hemorrhoid, this sort of swelling is very slow to disappear, and then leaves behind it a tab of loose skin.

"In the majority of cases requiring this operation your patient will claim the benefit of anæsthesia, or, if of the other sex, it will become you to recommend it, so as to spare her modesty, as well as to prevent possible pain. Here, not having the voluntary assistance of the patient in forcing down the hemorrhoids and presenting them for operation, you will be obliged to vary your mode of procedure very materially, or you will operate at a disadvantage.

"I have adopted, under these circumstances, the following mode of managing the patient, and it has succeeded so well in my experience—which covers now a large number of cases—that I can safely recommend it. I have found it of little use to have the patient force down his piles before the anæsthetic is administered, inasmuch as they are very apt to slip into the bowel again as the sphincter becomes relaxed; but I prefer that the patient should rinse out the bowel by an enema of tepid water, before he takes his place upon the couch or table. This latter should be firm, narrow, of convenient height, and in a good light. As soon as the patient is fully under the influence of the anæsthetic, I have him placed in Sims's position for operation on the uterus and vagina, that is, with the upper part of the body prone, the hips elevated, and the thighs flexed on the abdomen. There should be an assistant to take entire charge of the administration of the anæsthetic, and at least one more to aid the operator.

"I then commence the operation by thorough and complete *forcible dilatation* of the sphincter-ani muscle, by which the interior of the lower part of the rectum is placed entirely at my disposition, and afterwards proceed to the ligature of the hemorrhoidal tumours in the manner and with the precautions already described. The paralysis, or to speak more accurately, the atony, of the sphincter muscle, which results

from this manœuvre of forcible dilatation—which I will explain more fully another time—secures not only the great advantage to the surgeon of free and ready access to the lower part of the rectum, but it saves pain and trouble to the patient after the operation. The muscle does not recover the full vigour of its contractile power for a week if the manœuvre has been thoroughly accomplished, and meanwhile the patient is spared much pinching of tender parts. I have thought that retention of urine, which sometimes follows the operation for internal piles, has been prevented by it.

“As to the question of danger of the operation by ligature: in selected cases, it is so trifling as to be hardly appreciable. In between sixty and seventy operations, I have never had an unpleasant result. In over one hundred cases of operation by ligature, the late Valentine Mott had one fatal result—in a gentleman ‘who had just before met with great reverses in business.’ From the symptoms as recounted, I infer that the cause of death, in this case, was suppurative phlebitis, as he ‘fell into a typhoid condition’ shortly after the operation, and ‘small abscesses were found in the liver after death.’ I have knowledge of one other case where a similar result followed a partial operation; the patient, a medical man, having subjected himself to exposure and over-fatigue within a day or two afterwards.”

Dr. BODENHAMER uses “almost exclusively” a silk ligature; he declares

“It is seldom that my patients, during the whole course of treatment, are ever confined for a moment to either their rooms or their beds, but are enabled at all times to be up and attend to ordinary business.”

“The old method of tying piles, that which is recommended in the books taught in the schools, and usually practised, is (after the bowels have been evacuated and the tumours protruded as much as possible) to seize each tumour by either tenaculum or forceps, and draw it down fully out of the anus, and apply closely to the base of the part thus drawn down, a strong heavy silk or hempen cord, and then the same drawn and tied as tightly as can be; or a curved needle armed with a double ligature, is passed through the base of the tumour, so as to divide it into two, and the cords tied as tightly as possible on each side. After the tumours are all tied, they are returned within the anus, and an enema of starch and laudanum administered, the patient required in the meantime to maintain the horizontal posture, to live on meagre diet, and to avoid having any faecal evacuation for six or eight days. Sometimes immediately tying the tumours, they are then amputated closely to the ligature. This, in short, is the usual process now practised



in the ligation of hemorrhoidal tumours, and from the very nature of the case, must necessarily be attended with more or less danger, and with severe pain and inconvenience.

“The great objection to this method of operating is, the extreme suffering which follows and continues for a considerable time, and the confinement to either bed or room for several days. It is said, too, by some authorities, that the operation is attended with great danger from *tetanus*, *phlebitis*, or *pyæmia*, &c. This danger, however, has been, and is, in my opinion, greatly exaggerated. The few fatal cases reported were never verified by a *post-mortem* examination, and consequently are deserving of but little confidence. I believe when danger, extreme pain or failure attends the operation, it is generally referable to the unsuitableness of the ligature, and the injudicious manner in which it has been placed upon the tumour. I have, by my peculiar method, operated in thousands of instances, and have yet to encounter the first serious accident.

“The circumstances, then, of the danger, pain, and inconveniences attending the old operation, led me, about twenty-five years ago, to seek for information in relation to it, with a view if possible to remove some of the obnoxious features of it, or so to modify it as to make it less objectionable and serious, without at the same time rendering it any less efficacious in the cure of the disease. I first began by making some experiments upon both internal and external hemorrhoidal tumours, when in a quiescent state, expressly with a view to ascertain whether any one point or portion of the tumour was more sensitive than another; and more especially whether the mucous membrane, or other tissues from which such tumour proceeded, was more or less sensitive than the tumour itself or its covering. The experiments were conducted by means of a peculiar forceps, expressly made for the purpose. The apex of the tumour was first seized by the instrument, and firm compression made; the middle portion next, then the base, and lastly a portion of the lining membrane of the rectum, to which the tumour was attached, was included in the blades of the forceps. I found that in proportion as the compression reached the base of the tumour the pain was increased, and when a portion of the mucous membrane of the rectum or other tissue was included in the blades of the forceps, the pain was very severe. I have ever since, in operating, been very careful so to adjust the ligature as not to tie it too close to the base, and that nothing but the tumour itself should be included in its grasp. Now the question naturally arises, what is the cause of this difference in the sensibility of the natural textures from which the tumour proceeds, and those of the tumour or foreign growth itself? The most rational inference which occurs to my

mind at present is, that the former are more abundantly supplied with nerves and nervous influence than the latter. Be this as it may, however, the fact is as I have stated it, and it is in the power of any student to verify it. But on this, as on many other points of pathology and physiology, we are sometimes much better acquainted with the *quo* than with the *quomodo*; in other words, we know the facts, but we cannot well explain them. My researches on this subject have plainly taught me that so far as the natural tissues are concerned, the fine and delicate skin immediately without the anal orifice is the most sensitive; that the muco-cutaneous coat immediately within the anal orifice is next in point of sensibility, and that the mucous membrane of the rectum is the least sensitive of the three. I, however, have found the mucous membrane of the rectum much more sensitive than the mucous membrane covering the tumour. This must not be forgotten. Indeed, the foreign body and its covering, unless entirely external and covered with true skin, are much less sensitive than the three natural textures previously named.

“I now propose to offer some improvements in the operation of ligating hemorrhoidal tumours, the success of which has been invariable, and warranted by an experience of twenty-five years.

“By my method of operating, the tumour to be ligated is never seized by tenaculum nor forceps, and pulled down; for if this is done, a portion of the elastic mucous membrane of the rectum, to which the tumour adheres, also comes down with it, and therefore is almost certain to be included in the grasp of the ligature—hence the additional pain and suffering which necessarily follow; for the operator cannot distinguish the true base of the tumour from any other part when drawn down in this manner, for all the parts generally have the same appearance. I always require my patients to extrude the tumours simply by defecating efforts, or by the efforts produced by means of an aperient or a relaxing enema. If one or all these means should fail to protrude the tumours, I employ a bivalve speculum ani, introducing and arranging it in such a manner that the tumour which I design to ligate should fall between its blades; then, with suitable instruments, it can be ligated within the canal, just as easily as if it were extruded or external. I scarcely ever take up more than one tumour at one time, and never employ a heavy silk cord with a hard twist in it, such as saddler’s silk, which is the article often used for this purpose, but use a fine silk ligature, well waxed, with scarcely any twist in it, somewhat like floss or dentist’s silk; for in proportion to the size of the ligature and the hard twist in it, will be the increased pain it will occasion, and the length of time it will



take the tumour to slough off. As before observed, I am careful so to adjust the ligature as to exclude everything but the foreign body itself, and only make the ligature sufficiently tight to cut off the circulation—nothing more nor less. This can be known and adjudged by the appearance of the tumour whilst the ligature is being tightened. I am also careful not to place the ligature very close to the base of the tumour, as this produces more pain, and is not any more effectual in removing the whole of it. The small portion of the base of the tumour being below the ligature, will also sooner or later completely slough off. When the tumour is very large, or too large for one ligature, I divide it into two or more sections, according to its size, and multiply the ligatures, including but a small portion of the tumour in each. This is done by arming a suitably curved needle with a double ligature, passing it through the base of the tumour, and, if necessary, repassing it, and trying each ligature separately—thus including in the stitches every part of the tumour, and underlaying it, as it were, with a double uninterrupted suture. When part of the tumour is covered with true skin, or muco-cutaneous tissue, I usually incise this upon the same circle which is to receive the ligature afterwards, by which more or less suffering is avoided. I sometimes, when the tumour is entirely external and covered with true skin, and objection made to the knife or curved scissors, ligate it subcutaneously, which causes it to shrivel and gradually to disappear.”

Mr. SMITH, on the other hand, although he admits that the application of the ligature has proved to be admirable and permanent in its results, ardently advocates the use of the clamp, for as regards the ligature—

“ It was found that there were many disadvantages connected with it, and that it would be desirable if some other means of treatment equally efficacious could be put in force.

“ About thirty years ago Mr. Cusack suggested the employment of the clamp and cautery as a means of destroying hemorrhoidal tumours, and his practice was followed by other surgeons in Dublin. In this country Mr. Henry Lee adopted the method of using the clamp, cutting off the tumours, and then stopping the hemorrhage with the actual cautery. The results of this gentleman’s experience induced me to put the plan in force, and in the first cases which occurred in my practice I was so satisfied with the treatment, and so convinced of its superiority over the ligature, as regards the important elements of safety to life, freedom from suffering, and saving of time, that I determined to treat in this method those cases, both of hemorrhoids and prolapsus, where I thought the ligature was inadmissible, or where the patient objected to it, as well as those

wherein the application of nitric acid alone would not suffice to bring about a cure.

“I was not long, however, in finding out that the clamps hitherto used were capable of being greatly improved, for these were either of an awkward shape, were so constructed as regards the apposition of their edges, and so totally unfurnished with regulating power, that the efficiency of the instrument and value of the treatment were much impaired; so I suggested to Mr. Matthews to make me an instrument shaped somewhat like the ordinary clamp Mr. Curling uses for applying nitric acid: but the edges, instead of being serrated, or not meeting in their entire length, as in an instrument used by Mr. Henry Lee, were to be so constructed that they accurately fitted their whole extent by means of a groove on one blade and a raised surface on the other. I soon found the value of this, for the mucous membrane or tumour to be removed could be thoroughly compressed, and there was no fear of the bleeding which would take place when the clamps with serrated edges were used and the tumour excised. I next improved the instrument by furnishing it with a catch, and by this I was greatly assisted; but when the pressure was taken off the divided base of the tumour the cut surface would suddenly recede from the grasp of the blades, and if some vessel not thoroughly cauterised should bleed, it was difficult to get hold of the part again; so, to meet this emergency, I had a light but powerful screw added to the handles of the instrument, by means of which I could so regulate the power of the blades as to take the pressure off the cut surface of the rectum gradually, instead of suddenly, so that if any portion of the divided surface was not thoroughly cauterized the bleeding point would show itself, although still retained within the grasp of the blades, and by the slightest turn of the screw it could be secured and the bleeding vessel be entirely sealed up. This addition to the instrument I consider to be of the utmost importance, and, indeed, I never think of performing an operation without employing the instrument furnished with the screw.

“The operation, whether for hemorrhoids or prolapsus, is very simple, and consists of the following manœuvre:—The diseased portions, being well brought down previously by an injection, are separately seized with a vulsellum and handed to an assistant. The part is then enclosed within the blades of the clamp, which are screwed home quickly and thoroughly; the prominent portion of the pile or prolapsus is then cut away by a sharp pair of scissors, the cut surface is next dried by a piece of lint or sponge, and either the strong nitric acid or the actual cautery, so shaped as to come into contact with the whole of the raw tissue, is applied; when this is effected the blades are



gently and slowly unscrewed, and if there is no bleeding the part is well oiled and allowed to return within the cavity of the gut; if, however, any bleeding point is seen, the blades are quickly screwed together, and the cautery is applied until the vessel be thoroughly sealed up. The finger is then introduced well up into the rectum. This step serves the triple purpose of returning all the parts well, of compressing any point which might possibly bleed, and of exciting the sphincter to healthy action. I generally introduce at the same time a suppository of opium.

“Now, it might appear to some that this operation would be very painful, but, singular to relate, if great care be taken not to include any of the integument within the blades, and not to allow the nitric acid or cautery to come into contact with it, the patient does not feel much pain, and really does not know when the heated iron is being applied. If, however, the cautery be kept in contact with the blades of the clamp for more than a few moments, the patient will suffer pain by the transmission of heat through the contact of the metallic surfaces, and therefore it has been suggested to me to have some non-conducting medium applied to the under part of the blades. It is possible that I may make some alteration in this presently; but a grave objection lies in the fact that by this means the size and thickness of the blades would be materially increased.”

Mr. ALLINGHAM describes the mode of treating hemorrhoids by the ligature as applied at St. Mark's Hospital, after the manner devised by the late Mr. Salmon. Expressing his opinion that ligature is by far the best and most generally applicable method of operating upon hemorrhoidal diseases, he guards against being understood as referring to “the usual method of applying the ligature by transfixion of the base of the pile and tying it in halves.” The operation he recommends is as follows:

“The hemorrhoids are to be seized by the operator one after another with a vulsellum or pronged hook-fork, and drawn down; he then with a pair of strong sharp spring scissors separates the pile from its connection with the muscular and sub-mucous tissues upon which it rests; the cut is to be made in the sulcus or white mark which is seen where the skin meets the mucous membrane, and this incision is to be carried up the bowel, and parallel to it, to such a distance that the pile is left, connected by an isthmus of vessels and mucous membrane only.

“There is no danger in making this incision, because all the larger vessels come from above, running parallel with the bowel, *just beneath the mucous membrane*, and thus enter the *upper part* of the pile. A well-waxed, strong silk ligature is now to be placed at the bottom of the deep groove you have made, and

the assistant then drawing out the pile with some decision, the ligature is tied high up at the neck of the tumour as tightly as possible. If this be done, *all the vessels must* be included. The silk should be so strong that you cannot break it by fair pulling. A portion of the pile may now be cut off, taking care to leave sufficient stump between the ligature to guard against its slipping. When all the hemorrhoids are thus tied, they should be *returned thoroughly* within the sphincter; after this is done, any superabundant skin which remains apparent may be cut off; but this should not be too freely excised for fear of contraction on the healing. An injection of liq. opii sedativus may be administered. I always place a pad of wool over the anus, and a tight T-bandage, as it relieves pain most materially."

One very important practical point Mr. Allingham adds,

"I am quite convinced that the higher you carry your incision up the bowel the less does the patient suffer, because the ligatures are removed from the most sensitive part of the rectum and lie quietly above the sphincters."

In opposition to Dr. Bodenhamer as quoted, Mr. Allingham urges rest after the operation. He says

"I think it advisable, though not absolutely necessary, that the patient should keep lying down until the ligatures separate, which almost invariably takes place about the sixth or seventh day, occasionally a day sooner, very rarely a day later. If the ligatures are tied tightly and the incision has been free, this course of events is but very seldom departed from. *Active exertion*, even after the separation of the ligatures, is to be deprecated until the sores left in the rectum are quite healed; a fortnight or a little longer is generally about the time required to accomplish this. It is quite unnecessary that the patient should be kept in bed all this time, or even to his chamber—he may move about in moderation; but I am quite certain that a too speedy resumption of the erect position is likely to retard the cicatrization of the wounds."

Under five heads, advantages are claimed by Mr. Allingham for Mr. Salmon's operation as performed at St. Mark's Hospital:

"1st. The rapidity with which it may be executed. I have often operated upon four or five hemorrhoids, returned them, and removed redundant skin in one minute and a half or two minutes.

"2nd. There is only a very small amount of tissue included in the ligature; in fact, little more than the vessels supplying the tumour.

"3rd. At least three quarters of the wound is a simple incised wound which heals rapidly, only the small portion included in the ligature having to slough away.

"4th. The ligatures are tied a considerable distance from the



anus, so that, when returned into the bowel, they lie above the internal sphincter, where the sensibility of the mucous membrane is not acute, and consequently the pain and irritation after the operation is reduced to a minimum.

“5th. The operation is wonderfully free from danger to life, and its results generally are almost always satisfactory.”

Mr. Allingham combats Mr. Smith's statements in praise of the clamp and cautery, which he stigmatises as “extravagant,” and declares that he has seen a much greater proportion of untoward results from the clamp than from ligature, not only in his own practice, but in that of other surgeons; that the suffering after the operation is very considerable, the patient not being more free from spasm of the sphincter and retraction of the levator ani than after ligature; and that a good many times he had been annoyed after the clamp operation, to find that his patient had slight but recurring arterial hemorrhage.

The clamp, then, would appear to be admissible in those cases of internal hemorrhoids where the tumours are not numerous, and where especially there are no external piles or hypertrophic skin requiring removal. That the ligature is the safest and most effectual remedy, the opinion expressed by Mr. Curling in 1863, is thus supported by subsequent testimony.

The clamp has another antagonist in Mr. ASHTON, who declines to admit its advantages; he further adds that there is “the impossibility of including between the two straight lines formed by the jaws of the instrument, so irregular a growth as that constituting the base of a hemorrhoidal tumour, and which is also frequently attached as high as the upper margin of the internal sphincter.”

Mr. Ashton figures curved needles, having a cutting edge on the concave border, and furnished with two eyes, so that after the section of the anal attachment of the pile, the needle being driven through its base from without inwards, both eyes may be threaded with “ligatures of gold thread made of longitudinal strands of silk bound round by a spiral band of fine gold.” On the withdrawal of the needle, the two ligatures occupy its place, and can be tied, one on each half of the tumour transfixed. Mr. Ashton strongly impresses the necessity of drawing the ligatures as tightly as possible and of including the whole of the tissues affected, differing from Mr. Curling, who states that the contraction of the cicatrix is sufficient to reduce any part that may have escaped the ligature. Mr. Ashton thus follows the practice of Mr. Quain at University College Hospital. Mr. Ashton's needles probably afford a convenient instrument for landing the double ligature.—*British and Foreign Medico-Chirurgical Review*, Oct. 1872, p. 309.

49.—CASES OF STRICTURE OF THE RECTUM TREATED BY  
DIFFERENT METHODS—ONE OF THEM BY  
ELECTROLYSIS.

By Dr. WILLIAM R. WHITEHEAD, Physician for Diseases of  
Women at the North Western Dispensary, New York.

The unsatisfactory results which so often follow the ordinary treatment of stricture of the rectum, is a sufficient reason to call attention to the following cases. They illustrate in a comparative manner the value of certain modes of treating this disease, which is confessedly a very distressing one, and exceedingly difficult to manage. It is proper, however, to preface the relation of these cases by the statement, that I believe that the most effectual way to cure fibrous stricture of the rectum is by a thorough division of the stricture posteriorly, and by dilatation afterward by means of elastic pressure, such as I have described in a preceding number of this Journal (*American Journal of Medical Sciences*, Jan. 1871).

*Case 1.*—Mrs. F., a very intelligent lady about forty-two years of age, consulted me at the suggestion of Dr. Wm. George Thomas, of Wilmington, N. C. For a number of years she experienced much distress and difficulty in relieving her bowels; but was not aware of the presence of a stricture of the rectum until examined by Dr. Thomas about twelve months previous to the time at which I first saw her. He was of the opinion that the stricture was not cancerous, and this opinion was strengthened by a microscopic inspection by Professor Leidy, of Philadelphia, of some of the dejected matters which were sent to him for examination.

On the 15th of May, 1871, after she was thoroughly etherized by Dr. F. H. Deems, I made a careful examination of the rectum and contiguous parts. There was a tight stricture of the rectum more than an inch in length, and which extended from opposite the lower part of the Douglas cul-de-sac upward, and above it. A considerable mass of indurated tissue formed the stricture anteriorly, and occupied a very extensive relation with the peritoneal covering of the rectum. Mrs. F. informed me that she had for ten or twelve years worn a pessary in the vagina, and that she had once worn this instrument about a year without removing it. She was disposed to attribute the stricture to inflammation caused by the long-continued pressure of the pessary against the rectum, and I am quite inclined to adopt this opinion. The case appeared to be a formidable one for operation, and not being at that time so well convinced of the necessity of a thorough section of the stricture as I am at present, I was undecided whether to cut or rupture it.

However, believing it necessary to resort to either one or the



other of these procedures, in order to obtain subsequently a desirable degree of dilatation, with the elastic pressure, which I believe to be the safest method of dilatation, and an effectual one for the treatment of most cases of stricture of the rectum, on the 18th of May, assisted by Dr. J. B. Landeta, Dr. Francis H. Deems, and my present clinical assistant, Dr. A. Landeta, I ruptured the stricture with Sims's dilator, and completed the rupture with my index finger, having directed my force principally against the posterior wall of the gut. The rupture was made without difficulty, but I was apprehensive about the extent of this rupture and the danger of laceration of the rectum. This danger I do not believe to be overrated; and since I have quite satisfied myself about the safety with which a deep incision of the rectum may be made posteriorly, I prefer incision to rupture of a stricture except when seated quite near the anus. After the operation a piece of sponge about the size of a small orange and with a string attached was readily passed up the gut beyond the previous seat of stricture, so that the part could be well seen with Sims's speculum by all present. At the time of this operation, I was unaware of the existence a short time before, of a slight subacute peri-uterine inflammation. The knowledge of this condition gave me considerable solicitude about the safety of the patient. She had some peritonitis, which was controlled however with morphia in fractional doses given at intervals. The use of the duck-bill speculum, caused a fissure of the anus, which was cured by rupture of the sphincter ani muscle with the fingers. The peritonitis interfered with the immediate after-treatment by dilatation, but after six or eight days she used very effectively the instrument which I have recommended for elastic dilatation, but which has been so modified as to be much preferable to that first used. Four, five, or six rubber condoms one over the other, and properly adjusted to the instrument, will offer as much resistance as a stout Barnes's dilator, and are much easier to introduce either by the patient or by the surgeon. The probe which is attached to the instrument, and which is inside of the rubber bags, should be made of whalebone, and should be of the requisite degree of flexibility, and covered with a piece of rubber tube about the eighth of an inch in diameter. The tube protects the whalebone from the water, which would soon soften it and make it worthless as a director. There is no difficulty about the use of this instrument in the hands of an intelligent patient when once shown how to adjust properly the condoms to the instrument, and to its directing whalebone staff. A single rubber bag sufficiently stout would appear to be much preferable: but attempts have been made by instrument-makers of this city to make such bags for me for

this purpose, but there has always been some defect about them, which made them dilate very unequally, and burst at one of the seams or elsewhere. Rubber condoms or capotes can be obtained anywhere; they are used by oculists for ice-bags, and, introduced into the natural cavities of the body and distended with ice water, are useful for stopping hemorrhage. Half a dozen condoms, properly adjusted one within the other, and distended with water by means of a Davidson's syringe attached to a tube with a stopcock, I have found to be a good way to treat vaginismus after section of the vaginal sphincter, or even when cutting of this sphincter is not required.

It is very important to wash out the rectum three or four times a day with carbolized injections. A lacerated wound or cut of the mucous membrane of the rectum, constantly bathed with excrementitious matter and with pus, becomes without this means a great source of danger from septicæmia. During the latter part of July I received from Dr. Thomas a letter from which I make the following interesting extract:—

“The lower part of the stricture is decidedly relieved, the upper part not so completely. She has devised a modification of your capote dilator which struck me with force, and I think will answer an admirable purpose. She puts a silk bag between the rubber capotes, and ties them to the guiding staff. The silk bag may be stretched to its utmost capacity, and is firm, and is well protected from both the injected fluid and the secretions of the rectum by the condoms. It operates well, and upon trial I think you will be pleased with her ingenious improvement. She has used the dilator daily since she left New York. I advised her to use it once every three or four days only, during two to four hours at a time, and to gradually enlarge the silk bag.”

The width of the silk bag, a paper model of which Dr. T. sent to me in a letter, is two and a quarter inches. In a letter from the patient dated December 16th, 1871, more than six months after the operation, she stated that she uses the dilator quite frequently, and that her physician recommended her not to use it oftener than once in two or three weeks. She stated that her appetite was excellent, and that she was able to attend actively to her household matters; she had gained flesh, and all of her friends had observed and remarked upon her improved condition. We believe that a rather frequent, but judicious and long-continued, use of the elastic dilator in such cases to be very necessary.

*Case 2.*—Last summer Dr. A. Worthington, of Ontario, Canada, consulted me by letter relative to Miss M., who had stricture of the rectum of a number of years' standing, and which was complicated with a recto-vaginal fistula. The



parents of the lady, by advice of Dr. W., their family physician, requested me to visit her at her residence near the village of Wroxeter, in Canada West, and, if necessary, to perform such an operation on the stricture as the necessities of the case might require. Miss M. was thirty-four years of age; I found her anæmic, greatly emaciated, exceedingly inanimate and taciturn in her manner. I was much discouraged by the appearance of her general condition. Menstruation had ceased about two years before she consulted me, but previously it was regular. During two or three years there were discharges of bloody water from her bowels, together with lumps which her mother said resembled fat meat. During a period of four years she had occasionally hemorrhages from her bowels; altogether nine or ten in number. During the last three years her health had gradually declined. July 12th, 1871, the day on which I first saw her, assisted by Drs. Worthington and Byron Crandell, of Clifford, she was etherized and I made a careful examination of the rectum, which revealed by the vaginal touch a very indurated cervix and a recto-vaginal fistula on a level with the posterior lip of the cervix. The fistula admitted with some constriction the end of the index finger. The uterus was normal in size, but bound down posteriorly by adhesions. The rectal touch discovered two strictures with the recto-vaginal fistula between them. The lower stricture was inconsiderable in size, and was situated immediately above the sphincter ani muscle. The upper and principal stricture was seated three inches higher up the gut. The walls of the rectum between the strictures were firm, rigid, and posteriorly were slightly *nodulated* and suggestive of an interstitial, malignant deposit. The upper stricture admitted the end of the index finger. While the patient was under the influence of ether and in the semi-prone position, I tore the stricture posteriorly with the index finger of the right hand hooked into it. With two fingers of each hand I ruptured the sphincter ani muscle, and at the same time the indurated tissue of the rectal wall just above the sphincter. I then felt a fibrous band running somewhat obliquely across the posterior part of the upper stricture. I cut this band with a small knife which was concealed within, and could be projected at will from, a flattened sheath. The whole stricture immediately yielded, and by means of Sims's speculum, nearly the whole of the lower part of the rectum was plainly brought into view. The cut was made on the median line and posteriorly. There was not a teaspoonful of blood lost during the operation. The danger in this case was from peritonitis and septicæmia. The first I believe could be controlled by the appropriate use of morphia; the second could be prevented by washing out the rectum several times a-day with carbolized in-

jections. During the first twenty-four hours she was inefficiently nursed, and took the morphia very irregularly. She was allowed several times to sit up and strain at stool for one or two hours at a time, after which she was very much exhausted. When I saw her the next day, I urged the parents not to allow her to sit up, but to have a bed-pan put under her when required, and to procure immediately, if possible, an experienced nurse. During the second night after the division of the stricture she had an alarming collapse, and vomited once. She was resuscitated by the application of hot bricks to her feet and by teaspoonful doses of brandy and water. I remained at the house with her during the night and part of the following day.

On the 14th of July a good nurse was obtained, and the pulse, which had been at 140, under the influence of a solution of sulphate of morphia regularly given fell to 120, and the patient's condition was better. There was not so much tenderness about the lower part of the abdomen. The mother persisted occasionally in giving the patient her medicine, and instead of a teaspoonful of a solution of morphia which contained the one-eighth of a grain of this substance, gave by mistake a teaspoonful of a saturated solution of carbolic acid. This was used largely diluted with water to wash out the rectum. Dr. Crandell was in the room at the time that this mistake occurred, and Dr. Worthington and myself were in the adjoining room. The whole interior of her mouth was blanched, and she experienced very great distress in swallowing some milk which I requested her to drink immediately. After a few hours the pulse ran very high, and the distressed feeling about her mouth and throat increased, and she died about twelve hours after the swallowing of the carbolic acid, which contained only five per centum of water. I regarded this case as a very unfavourable one, and which possibly under different circumstances I should have declined to operate upon. But it was quite evident that her only chance to obtain any relief was in a careful division of the stricture or by its rupture in the manner in which I effected it, to be followed by methodical dilatation with the rubber bag distended with water. She had commenced under better nursing rapidly to improve, and would I believe have recovered had she not swallowed the carbolic acid.

*Case 3.*—The following case is interesting from the fact that several attempts were made to disperse, by electrolysis, the interstitial deposit which formed an indurated mass that caused the stricture of the rectum.

Mrs. K. consulted me at my clinic at the Northwestern Dispensary more than eighteen months ago, for stricture of the rectum. The stricture admitted with difficulty the end of the finger, and extended from opposite the Douglas cul-de-sac,



upward above it, and beyond the reach of the finger. The stricture was unmistakably the result of syphilitic ulceration of the rectum, and the patient had a secondary eruption on the skin. This eruption, under appropriate treatment, disappeared, but reappeared once or twice, and she, at present, has an ulceration of the leg of syphilitic appearance. Soon after I first saw her, she was etherized, and the stricture was cut with a curved bistoury anteriorly, and posteriorly at its lower part; but as I could not reach the upper part of the stricture with my finger, the operation was necessarily incomplete. The knife was securely wrapped, except near its point, with cotton, and the cutting part was well protected by the end of the index finger of the right hand. Attempts were irregularly made to dilate the stricture with the rubber bags, and by forcible dilatation with the finger. I held in reserve, however, this case for another cutting operation, and decided that I could safely make a very extensive incision, if required, on the posterior wall of the rectum. My opinion in this matter was strengthened by a careful examination of two or three cadavers with reference to the relations of the peritoneum to the rectum. Besides, Professor Darling, of this city, professor of anatomy in the University Medical College, kindly called my attention to a dried specimen which well illustrated these relations in a manner usually overlooked by anatomists. An incision made posteriorly, at any point along the median line, from just below the promontory of the sacrum, and between the folds of the mesorectum to the sphincter ani, will not wound the peritoneum, or be likely to be followed by hemorrhage. But the peritoneum covers the sides of the rectum quite low down, and the peritoneal duplicature of the utero-sacral ligaments in relation also, anteriorly with the rectum, may, especially in partial prolapsus of the uterus, draw the peritoneum unusually low down on the sides of the rectum.

I was attracted to the use of electrolysis in the treatment of strictures of the urethra by Tripier's article in the *Arch. Gén. de Médecine*, and besides, I was invited by Dr. Robt. Neuman, of this city, to see with him a patient, with stricture of the rectum, and to which he made one application of electricity with Stöhrer's battery of sixteen elements, and subsequently used my dilator for elastic pressure. I am not quite sure which electrode Dr. Neuman applied to the stricture. As an immediate result of this application the stricture was considerably dilated. Quite recently a post-mortem examination of the case afforded Dr. Neuman an opportunity to exhibit to the New York Pathological Society the rectum of this patient. It seems that, having obtained some relief, she became careless and neglected herself; finally she got worse, and had fecal fistulæ to

form. The specimen shown to the Society interested me exceedingly, but I was unable to draw any definite conclusions from the appearance of this specimen, as to the value of electrolysis in the treatment of strictures of the rectum. However, I had, after having seen Dr. Neuman's case, obtained Dr. Wm. B. Neftel's co-operation in the electrolytic treatment of the stricture of Mrs. K., the subject of the third case, and which I am now reporting. The electrolytic treatment of this case extended over a period of one month, and during this time four applications of electricity were made. All other kinds of treatment were purposely avoided, so that the electrolytic action might be well tested. The patient was not etherized, and the first application was made with a new galvanic battery of Curt Mayer, of New York (a platina zinc battery). A cathode made for the purpose, consisting of hard rubber and a solid brass cylinder rounded at the end, was introduced by me partially into the stricture. Dr. Neftel commenced with one element, and increased gradually, one by one, to twenty elements. There was a slight and gradual increase of pain. A current of twenty elements was maintained for fifteen minutes. I repeatedly examined the part during this time. The applications lasted about half an hour. There was an immediate dilatation of the lower part of the stricture only.

On the 16th of January, 1872, a week after the first application, another one was made with Remak's battery. Dr. Neftel thought that it would be well to isolate with shell-lac, or hard rubber, the rounded end of the brass cathode. It was isolated at its end with shell-lac, and introduced by me into the stricture. The anode, in the shape of a flat disk, and separated from the skin by a piece of wet cloth, was, as in the preceding application, applied to the hip, and shifted about from place to place, when the heat developed at the anode, or positive pole, became unendurable. The application began with two or three elements, and was gradually increased to fifteen, at which number the current was maintained for five minutes, with a deflection of the needle of the galvanoscope of fifteen degrees. The current was then gradually, by the addition of one element at the time, increased to twenty-one elements, as denoted by the position of the winch in the current-selector. A current of twenty-one elements were used during five minutes; it was then interrupted, and I examined with my finger, and found that the upper part of the stricture was considerably dilated. The circuit was again closed, and the cathode re-applied to the central part of the stricture. The applications this time lasted altogether fifteen minutes, and the patient afterward did not feel uncomfortable. Possibly it may be well to observe that Dr. Neftel used the elements of Siemens, which were substitu-



ted for Remak's elements, and were contained in the box of Remak's apparatus. It appears to me that, for accuracy of observation, it would be desirable to adhere to the use of one apparatus.

January 24, a third application was made. Previously the patient stated that her bowels were more easily moved, and an examination revealed that the stricture was more dilated. A brass cathode, isolated at its end with hard rubber, was introduced into the stricture, and the anode applied to the external part of the thigh. Twenty-three elements of Siemens were used during twelve minutes, with a deflection of twenty-three degrees of the needle of the galvanoscope. Twenty-one elements were used for eight minutes longer. A considerable quantity of frothy mucus, caused by the evolution of hydrogen at the cathode, issued from the rectum. The applications this time lasted altogether twenty minutes. After the discontinuance of the current I found that the upper part of the stricture was much dilated, but that the middle part was still constricted. A week later, another application of electricity was made much in the same manner as this one, but of which I failed to keep any notes. I decided not to use dilatation after these applications, but to permit a few months to elapse before I examined her. I believe this was also Dr. Neftel's suggestion.

April 16, 1872, she came to the Northwestern Dispensary, and was carefully examined by my clinical assistant, D. A. Landeta. The stricture with difficulty admitted the end of the index finger, and was quite as tight as before the electrolytic treatment.

Possibly it may not be out of place, in connection with this case, to attempt to define what is meant by electrolysis as applicable to the dispersion of certain tumours, or of the induration of the tissues, as in stricture. The different effects of the two electrodes on the living tissues should be distinguished one from the other. These effects are modified by the use of batteries of great electro-motor power, or of feeble power, or composed of many small elements, but of great tension. It seems to me that a multitude of different circumstances influence the effects of the currents. The kind of electrode used; such as needles, blunt or flat electrodes, and these last with or without moisture, modify the effect. The moving of the electrodes, and, if needles be used, the position in which they are stuck into the flesh, whether parallel or in a line continuous with each other, are also worthy of attention. The effects must necessarily differ widely, because, if, as in the last case, the needles intersect, or are continuous with each other, especially should their points be near together, we may have a bad slough of the

tissues in the place of a resolute or electrolytic action, such as is supposed to occur at the cathode of a galvanic current, under conditions favourable to the development of the peculiar effects of the negative current at this pole. It is not at all my object to discuss the subject of electrolysis, but I, probably in common with many others, would like to know what is its practical value, and also something about the right indications for its use, and the proper manner to develop the electrolytic action.

It is necessary, it seems, to distinguish well the decomposing power of the current from its heat-producing power. The chemical and decomposing power, or electrolytic effect, is supposed to take place at the cathode, and is caustic in its action, but caustic after the manner of alkaline caustics. The heat-producing power of the current is developed at the anode, at which may be produced a disorganization of the tissues immediately around it, similar to that caused by the actual cautery. If the anode be a thickly plated gilt needle, thrust into the tissues, it will be corroded by oxidation, and there may be considerable difficulty in withdrawing it. The disorganization of the tissues immediately around the needle will be proportional to the greater or less intensity of the current. If the cathode, or negative electrode, be also a needle, and thrust into the flesh, no corrosion of this needle occurs, but soon the tissues around it will become emphysematous by the escape of hydrogen. An alkali is evolved at this electrode, and an acid at the other. It would seem that there occurs a mechanical disassociation of the tissues by the action of the escaping hydrogen, and a dissolving and fluidifying effect caused by the alkali liberated at the cathode. If the anode be a plate or disk, the heat may be so great as to burn the patient severely. It should be recollected that the positive electrode is at the end of the wire which leads from the negative metal, and the negative electrode at the end of the wire which leans from the positive metal, and which would be the zinc, if this be used, in a zinc and copper battery.

The direction of the positive current is from the metal oxidized to that which is not, and the direction of the negative current is the opposite. Tripier's experiments on strictures of the urethra gave immediate results which were seemingly satisfactory. The strictures in each case after the application of the cathode were enlarged, but their condition a few months afterward is not stated. He concludes that the negative galvanocauterization, or that obtained with the cathode, is an excellent substitute in certain cases, for the caustic of Filhos, and that the cauterization obtained with the anode may replace that of the strong acids, or the actual cautery. He claims that the cicatricial tissue which results from cauterization with the cathode is soft, thin, and not resilient, and that the inodular tissue pro-



duced at the anode has the usual character of such tissue. The greater the electro-motor force of the battery, the more rapid, and the more painful will be the cauterization. Whichever electrode is selected for cauterization, the effect would be, apparently, to burn a larger hole through the stricture, and it seems to me that dilatation should be used afterward in order to obtain any benefit from this mode of treatment.

*Case 4.*—As an example of stricture of the anus and lower part of the rectum, caused by the injudicious application of caustics for the cure of hemorrhoids, the following case may prove interesting.

Through the recommendation of Dr. Horace P. Farnham, of this city, a gentleman from one of the Western States recently consulted me for an intolerable itching about the anus, and for great difficulty in evacuating his bowels. There were hard and slightly elevated folds of the integument immediately around the anus, and on introducing the finger into the rectum, the tissues about the anus felt very much indurated, and formed a tight stricture at the anal orifice. On remarking to him that probably some caustic application had been made to the parts, he replied that a few years ago he was treated for hemorrhoids by several applications of caustic, and that he had since noticed a gradually increasing difficulty in relieving his bowels. I told him that gradual dilatation was, in his case, the only safe and effectual means of relieving him, that division of the sphincter ani, at present at least, was unnecessary, and that dilatation would also afford him relief of the itching. I advised him to have a number of smooth and slightly conical plugs, about two inches long, made of maple wood, each with a handle, and also with a flange, to prevent it from slipping too far into the rectum. These plugs should be graduated in size, from that of the finger to that of the largest bougie.—*American Journal of Medical Sciences*, July, 1872, p. 114.

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#### 50.—ON REMOVAL OF THE TONGUE FOR EPITHELIAL CANCER.

By JOHN WOOD, Esq., F.R.S., Surgeon to King's College Hospital.

Up to the present time the operation for the removal of the tongue or any part of it has been considered by most surgeons as one of those in which the blessings of anæsthesia could not with safety be afforded to the patient, for reasons which will be at once apparent—viz., first, the removal by the anæsthetic agent of the voluntary assistance which the patient can render in all operations upon the interior of the mouth, by keeping the jaws and mouth wide open and the tongue

steady; and secondly, the supposed danger, in profuse bleeding into the pharynx, of suffocation by the blood passing into the larynx and trachea. It is very evident that if we cannot in these cases offer to our patient a means of relief unattended by immediate pain, and that of a most severe character, at least half of the advantages of, and inducements to, the treatment involving such an operation are thrown away, and patients will be naturally indisposed to listen to the only promise of relief which can honestly be held out to them. If, then, we can render by any means the necessary operations painless, we confer, I think, a boon upon humanity not by any means small to the poor creatures thus afflicted.

By the use of an instrument made for me by Messrs. Matthews, we obtain a perfect and steady command over the jaws, without encroaching upon or interfering with the cavity of the mouth. It consists of four teeth plates, which act on both sides upon the upper and lower bicuspid by means of a pair of powerful cross levers, placed outside the cheek, and opening independently of each other by a screw on each side through a simple hinge joint. Combined with these levers as they bend into the mouth is a groove to receive the angles of the lips, which are thereby kept wide apart, and out of the way of the light, which is so necessary in securing bleeding arteries in a deep cavity like the mouth. The two sides of the instrument are connected and kept together by a bent piece of metal, which passes under the chin altogether out of the way of the operator, and which is capable of being adjusted to the width of the lower jaw by a sliding groove and fixed by a screw. The whole is held firmly upon the patient by a strap passing round the back of the neck and buckling on to the opposite piece. This instrument has been adopted after a great many trials of different kinds of gags, including that most valuable one for operations upon the palate invented by Mr. Thomas Smith, but which, from the embarrassing presence of the tongue-plate, proved inadmissible in operations upon the tongue. In some respects the present instrument resembles this, and the differences are mainly such as refer to leaving free the tongue and holding the lips well stretched out. I may add that the gag is equally effective in operations upon the tonsils and larynx, and that Sir W. Fergusson employs it, with entire satisfaction, in cases of staphyloraphy.

More difficulty than you would imagine has attended my efforts to get a gag that could be depended upon to resist the struggles of a patient under chloroform, and to meet the ridiculous ease with which the patient, by simply opening his jaws a little wider than the angle at which the gag has been fixed, and at the same time pushing with his tongue and cheeks, as in



the act of spitting violently, which so commonly occurs under chloroform, can render nugatory all your precautions to secure a full command over his month. The ordinary dentist's gags are often more than embarrassing and worse than useless in these cases. One of the most frequent difficulties arises from the absence of the teeth so commonly drawn to prevent irritation of the tongue on the affected side in these cases. The independent action of the two sides of the gag just described enables one to be opened wider than the other to meet the extra width required by the absence of teeth. A one-sided gag is much more easily displaced by the patient and is more difficult to fix than a double-acting one.

Having thus obtained the necessary control of the movements of the patient's lips and jaws during the insensibility produced by chloroform, we have only to look to those precautions against bleeding and the passage of blood into the larynx which are usually found most efficacious, such as turning the head down to one side and clearing the throat with the finger. If the knife be used the tongue must be held well out of the mouth by a good-sized vulsellum forceps, with hooks sufficiently large and widely placed to prevent their tearing out of the sound fleshy substance of the tongue into which they should be inserted. If the tumour is likely to be very vascular, as may be in some measure divined from its appearance, I have usually passed first a stout ligature across the ranine arteries under the base of the tongue, by means of a curved needle in a handle,—the one, in fact which I devised for the performance of the operation for the radical cure of hernia. This controls the hemorrhage until the cut ends of the vessels have been secured. The tumour is next seized by a pair of stout double-toothed lion forceps, such as those most useful ones which I now show you, and which we know in this theatre as Wilcox's forceps. I have found these by experience to be less apt to slip than any other I have tried. Then a curved, sharp-pointed bistoury, to transfix the tongue close to its base, and to cut first antero-posteriorly and then laterally across the vessels and nerves, will suffice rapidly to clear away the diseased mass. In securing the arteries much steadiness on the part of the assistant is required. The muscular substance of the tongue is so friable under the circumstances that it is readily broken down by too much vigour in drawing the knot; while the depth at which the cut vessels lie from the surface necessitates dexterity in slipping the thread over the forceps, the points of which should be conical and taper quickly to the end. The proper tying of the vessels, and the prompt application of the tincture of the perchloride of iron and chloride of zinc by a strip of lint tied round a stick, will render the use of the actual cautery

rarely necessary to subdue the hemorrhage. It is astonishing how rapidly the patients recover after this operation; and, if too much blood has not been lost, how soon they regain strength and a more healthy appearance. Freedom from pain, undisturbed rest at nights, improved appetite, and revigorated digestive powers account for this satisfactory result.—*Lancet*, Sept. 28, 1872, p. 441.

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#### 51.—TWO NEW FORMS OF ENEMA APPARATUS.

By W. L. SHEPARD, Esq., late Tutor to St. Bartholomew's Hospital.

Most of the enema apparatus hitherto invented present some disadvantages which militate against their usefulness. The ordinary brass enema apparatus is cumbersome and extremely inconvenient to use alone, as it needs both hands to work it, so that it is difficult to keep the canula *in situ*. Again, if not often used, the packing on the piston is sure to get very dry, and, in hot climates especially, the instrument is in this way often rendered useless, it being no easy matter to pack a piston properly.

The irrigateur of Dr. Eguisier is easy to manipulate, but it is very bulky, very liable to get out of order, and could only be repaired by an instrument maker. Perhaps the best form of apparatus yet made is either the Higginson or Kennedy. They are both of them portable, and can be used with one hand, but they have also both of them one radical defect—namely, the vulcanised india-rubber ball. However good this may be when first manufactured, after a time it undergoes some molecular change and becomes worthless. Again, if the ball be made thin it fills but slowly, and if made thick it is very tiring to the hand; but more than all, oil or turpentine (so often used in enemata) soon destroys the india-rubber.

I have endeavoured to remedy all these various defects in the two forms of enema apparatus I have had made for me by Messrs. Arnold, of West Smithfield, E.C. I propose to call one the Simplex, and the other the Facilis.

The Simplex consists of two cylinders, sliding easily the one within the other. The upper and smaller of the two is closed at the top, and is kept raised by means of a spiral spring which rests against the bottom of the lower cylinder. This latter is closed at its lower end by the solid base on which the instrument stands, and its upper end is made of rather larger diameter than the rest so as to form a circular chamber, which has holes pierced at its under surface. An inlet valve is fitted into the base of the lower cylinder, and an outlet valve to an



opening leading to a space between the inner and outer surface of the lower cylinder, which space is continuous with the discharge-pipe.

To use the apparatus, it is placed in a basin or other vessel containing the liquid; the upper cylinder is pressed down by the hand, and then allowed to rise again by the action of the spring. It thus acts as a piston, and the fluid is drawn through the inlet valve into the lower cylinder. On again depressing, the greater part is forced through the outlet valve, and the little that escapes between the cylinders is forced into the circular chamber, whence it runs through the holes on its under surface back into the vessel.

The Facilis is made essentially on the same principle, but it is constructed to feed itself from any vessel, and is intended to be used in cases where it would be inconvenient to stand the apparatus in the fluid: for instance, when it is required to be used in bed, or with delirious or unruly patients. It consists of a cylinder having the upper part of larger diameter than the lower, and fitted with a watertight cover, which is pierced with an opening fitted with a stuffing box. Inside the cylinder is a loosely fitting metallic piston without packing, kept pressed against the cover by means of a spiral spring. In the centre of the upper surface of the piston is a depression corresponding with the opening in the cover. The piston-rod, which is loose, drops into this. An inlet valve is fitted to an opening in the lower part of the cylinder, to which the feed-tube is attached. The upper tube is fitted to an opening on the under surface of the enlarged part of the cylinder, and acts as a waste-pipe. An outlet valve is fitted to the opposite side of the cylinder, and is connected with the discharge-pipe. To use this apparatus, the feed-pipe and waste-pipe are both allowed to dip into the vessel containing the fluid. Then, on alternately depressing the piston and allowing the spring to raise it, the liquid is drawn into the cylinder through the inlet valve, and forced through the outlet valve. What little makes its way beside the loosely-fitting piston goes into the large upper part, and runs through the waste-pipe back into the vessel.

The advantages which I think these apparatus possess are—  
1. They are very easy to use, needing only one hand to work them. 2. They are eminently portable, the Simplex being only four inches high and an inch and three-quarters in diameter at its widest part. The Facilis is three inches high and two inches in diameter. 3. There is no packing on the piston; and they are so simple in construction that it is next to impossible they should get out of order. 4. They can be taken to pieces to clean, and put together again in a few seconds, and without

trouble. 5. They are made entirely of metal, and the springs are of plated steel, so they are not likely either to rust or become weakened.—*Lancet*, August 17, 1872, p. 222.

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## ORGANS OF URINE AND GENERATION.

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### 52.—ON LITHOTOMY.

By Professor HUMPHRY, F.R.S., Surgeon to Addenbrooke's Hospital, Cambridge.

[In lithotomy, as in all other surgical operations, the great secret of success is a painstaking and careful method of procedure. The minimum of deep cutting is the safest course, and gives the best results. It is far better to do the operation very carefully and be a little longer over it, than to do it rapidly and with apparent dash.]

A good case for lithotrity is one in which lithotomy, carefully performed, is attended with very little risk, with quite as little, I believe, as lithotrity. It is less painful, quicker, and more certain; and the greater number of our patients prefer this more expeditious process. I use the common staff with the groove on the convex side in preference to one in which the groove is placed laterally, as recommended by Mr. Liston, because the groove, when on the convex side, is more easily felt in the perineum, and the knife is more easily pressed into it and is less likely to escape from it. A common scalpel is quite as good as any of the variously formed knives which have been employed in this operation; and it is advisable to adhere to a few instruments, just as it is advisable to adhere to a few remedies. By using many of either we merely dilute and waste our experience. The man who uses a few weapons is more likely to wield them skilfully than he who tries, and so perplexes himself with, many. Having made a tolerably free incision through the skin and superficial fascia, I pushed my left forefinger through the tissue of the perineum, down upon the staff a little behind the bulb, so that I could feel the groove of the staff sufficiently to guide the point of the knife upon my finger-nail into the groove. I then cut a little way along the staff, withdrew the knife, examined with my finger, and, again introducing the knife as before, cut a little more, repeating the process till the part of the prostate nearest to the surface was divided, and till I could insert my finger, alongside the staff, through the prostate, into the bladder. Thus the opening was dilated. It was further dilated by my passing the forceps, beside the finger, into the bladder, which I did after having explored the bladder



and felt and ascertained the size of the stone. I make a point of doing this, as it greatly facilitates and gives safety to the further steps of the operation, and, having the advantage of a long finger, I am almost always able to examine the size and position of the stone, and select the forceps accordingly. These should be so made as to hold the stone securely and not let it slip from between the blades; and the blades should be broad, so as to cover the stone and prevent its sides catching and rubbing roughly against the tissues as it is drawn through the wound. In the present instance the forceps did not open easily in consequence of the hinge not being quite free. To have used force in opening these might have endangered the wall of the bladder; so I at once withdrew them and resorted to the scoop, which I guided upon the finger again introduced, and with which the stone was quickly removed. An elastic tube, guided upon the finger into the bladder, was secured there, and removed on the third day. The man has not had the slightest pain or discomfort since the operation, and is doing well.

The points on which I lay most stress are, the pressing the finger down upon the staff in the membranous part of the urethra and guiding the scalpel by it into the groove, and careful cutting into the prostatic part of the urethra, and the completing the operation with the finger and the forceps or the scoop. I rarely do more cutting than this. If it is a large stone, I divide the prostate a little more freely with the knife, and then allow the wound to be dilated by stretching or tearing of the tissues as the stone is drawn through it, which I do slowly and steadily, though I have sometimes to use considerable force. By this plan there is no greater division of the tissues than is absolutely necessary, and there is less danger of ill consequences than if the knife is resorted to. I ought to add that it is necessary to be very careful not to allow the finger, still less the knife, to stray into the loose tissue on the exterior of the prostate. Particularly is this care needed in operating upon children. I am sure that many of the disasters which have occurred in the operations upon young subjects have been caused by the finger pushing its way on the exterior of the prostate and bladder, where the tissue easily gives way before it, and where a cavity is easily made by the finger, which the operator mistakes for the bladder. Into this he plunges the forceps, perhaps seizes the bladder or the rectum, or does some fatal mischief. In a little child it is sometimes difficult to enter the bladder. The tissues are soft, and readily give way before the finger; the bladder seems to be at an unexpected distance, and the operator is unnerved by the difficulty. I can give no more valuable hint than that of preparing a young operator for this difficulty, and of warning him to be content to go slowly

on, not to be seduced into quitting the guidance of the staff, not on any account to withdraw the staff till he is certain that his finger is in the bladder, and till he feels the stone with it, but to work quietly and gradually on with finger and knife till he has brought the finger into contact with the stone. Indeed it is a good rule, from which I hardly ever depart, not to allow the staff to be withdrawn from the bladder till the finger is upon the stone, and not to withdraw the finger till the forceps or scoop is in the bladder. I do not use a very large staff, because it increases the difficulty of passing the finger along it through the prostate. I introduce it with a slight wriggling, rapid, but gentle movement. I am not satisfied unless I hear it strike the stone as well as feel its contact with it. The ear is in this case a better guide than the finger; and if this were more attended to, the cases in which a stone is not found would be fewer. The touch is often deceptive, especially in children, in whom the projecting and slightly roughened rugæ of the bladder frequently communicate, when the sound is passed quickly over them, a sensation to the finger like that given by a stone. I was present not long ago when an operator opened the bladder and could not find a stone. To my inquiry, two or three times repeated, "Did you hear the staff strike upon the stone before commencing the operation?" the reply was, "Oh, we felt the stone quite distinctly; there was no doubt of that!"

Rules in surgery, however, must sometimes be departed from, and judgment and skill may be more shown in the breaking than the observing them. A lad was in the hospital two years ago with severe symptoms of stone, the urine constantly passing. The sound appeared to grate upon something, but we could not hear it strike a foreign body. Several trials gave the same result. The symptoms were so bad that it seemed best, at any rate, to open the bladder and search for the cause. Accordingly, having introduced the staff as usual, and perceived the grating, I cut carefully upon it and along it quite to its end. I could not discover a stone, and I soon became certain that neither my finger nor the staff was in the bladder. The staff, being no longer of any use, was withdrawn. Then my finger touched the end of a rough stone. Supposing that this had escaped from the bladder into the surrounding tissue, I was endeavouring carefully to extract it when it disappeared; and my finger, following it, passed through what was evidently the prostatic part of the urethra into the bladder, and felt the stone there. It was removed without difficulty. It is, as you see, composed of two oval parts, united at an angle of about 100 degrees. Of these one has a nucleus of lithic acid, and lay in the bladder; the other consists of phosphate of lime, and lay in a cavity or urethral pouch outside the bladder; and the



bent neck joining the two occupied the prostatic part of the urethra. The staff, no doubt, had passed into the urethral pouch, beneath the calculus, had probably penetrated the delicate wall of the pouch, and found its way into the cellular tissue behind the bladder, into which I travelled along its groove with scalpel and finger. The stone lying in its concavity was not discovered by my finger till the staff was withdrawn. The boy quickly recovered.

You observed last Friday that I quickly relinquished the forceps, and resorted to the scoop. This is an excellent instrument and very safe, because it does not go much beyond the range of the finger. It is not trusted in the bladder without the finger, and is not opened and shut and moved about in the bladder so freely as the forceps are; and, with a little practice, the stone is very easily caught between it and the finger, and drawn through the wound. Even large stones may be thus removed. This stone weighed eight ounces. It is the largest I have ever seen extracted by lithotomy. It is nearly spherical, is rough on the exterior, and was grasped by the bladder so that I could not lay hold of it with the forceps, and should probably have failed to complete the operation had I not fortunately taken with me (it was several miles from Cambridge) this strong well-bent scoop. I contrived to insert it between the bladder and the stone; and it held to the stone so well that with it and my finger I succeeded in gradually drawing the stone through the wound. The patient, a stout farmer, aged sixty-five, recovered without any unfavourable symptom. The handle of the scoop should be large and rough, and the stem round and strong. In some of these scoops I have had the bowl set sideways on the stem, which sometimes facilitates the catching of the stone between it and the finger.

With regard to hemorrhage: I am in the habit of telling my pupils that hemorrhage after an operation is almost always attributable to some fault on the part of the surgeon. It indicates that he has not been sufficiently careful in searching for the bleeding vessels, or that he has not properly secured them. This is, at any rate, a very safe doctrine to inculcate; and it applies to lithotomy as well as to other operations; for the vessels which bleed most briskly, and are most likely to give trouble after the patient is placed in bed, are commonly within reach, and may, with care, usually be found and tied. They are the artery of the bulb, or the transverse artery, or some branch divided near the pudic; any of which may be tied. After the removal of the stone I always attend to this point, and sponge out and explore the wound carefully, if there is any bleeding which is likely to give trouble. When the hemorrhage is from a deeper source than I have indicated—that is, from the neigh-

bourhood of the prostate—it cannot always be arrested; but in such cases it usually ceases spontaneously after a short time, there not being commonly any arteries in this situation sufficiently large to keep up, or to cause a return of, the bleeding.

I leave an elastic tube, passed through the wound, in the bladder, in men, to avoid the difficulty which is sometimes experienced by the patient in voiding urine after the operation. I do this because I was, in several instances, summoned after a few hours, in consequence of the patient being in severe pain and unable to pass urine; and I was obliged to introduce my finger into the bladder to afford relief. This trouble, which was perhaps a consequence of the small size of the incision I am in the habit of making into the bladder, is quite prevented by the tube. The tube may be removed in two or three days, or earlier if desirable, and is not again required.

In one instance, a gentleman, aged sixty, in whom the operation was unattended with any particular circumstances, there was rather severe inflammation of the bladder ten days after the operation, with discharge of tenacious and semipurulent mucus. For several days I introduced, two or three times a day, an elastic catheter by the urethra into the bladder, and injected through it warm water, which, passing out at the wound, washed out the bladder. The symptoms subsided; and he was soon quite well, and has remained so. In another case, when the wound was healing, a patient, a farmer, aged sixty-four, had retention, and required the catheter to be introduced two or three times daily. After a time he quite regained the power. In a third patient, a middle-aged man, in the hospital, the symptoms of bladder irritation set in while the wound was healing, and increased in severity, so that I introduced a sound, and discovering a calculus, I reopened the wound, partly with the knife directed by the staff, and partly with the finger, and removed a rough phosphatic calculus, which I have no doubt had formed in the interval since the first operation, when the stone removed was lithic acid. I mention these cases because it is possible that the symptoms may have been dependent upon the incision in the prostate having been small, and the parts having, in consequence, sustained some injury in the extraction of the stone. I do not know that it was so; and if it was, the occasional occurrence of such events would not induce me to deviate from the practice I am in the habit of pursuing.

With regard to the after-treatment, it is very seldom that any is required. The diet should be, not according to any rule, but regulated by the condition and the desires of the patient. He usually prefers to limit himself to tea, broth, &c., for a day or two; and this generally suits him best. Then appetite returns gradually, and may be yielded to. We



do not often find that wine is necessary. The man now under treatment, as I just said, has had no pain or discomfort whatever. Generally some pain is experienced when the urine flows, or at other times; but whether there is much pain or little, whether the patient sleeps or does not sleep, I refrain from giving opium or any other sedative. Pain, even severe and long-continued, though hard to bear, does not seem to do the body much harm. I have often been surprised how little wear and damage it does—not so much, I think, as the sedative which is given for its relief; and the exhaustion of a sleepless, restless night or period is usually followed by sleep, which is more likely to occur naturally at an early period if sedatives have not been used. Under ordinary circumstances a restless night is less damaging than a night of sleep induced by sedative. The patient commonly wakes from the latter paler, weaker, lower, more enervated, and less able to bear pain. I have long had this conviction; indeed, throughout life I have acted upon it. It may be regarded as a prejudice; but I think the patients recover better and quicker, and are less liable to pyæmia, erysipelas, and other unfavourable sequelæ of operations, when thus treated. These, at any rate, we rarely see in Addenbrook's Hospital; and an immunity from them is, I think, partly due to the practice of non-interference after operations so generally followed in this hospital. You will understand that I make a distinction between the use of opium as a remedy for disease and as a means of relieving pain after operations. In the former case it is a very valuable medicine under many circumstances, one of the most valuable we possess. But in the latter case there is no disease; the pain is merely a natural consequence of the physical injury which has been sustained, or of the local changes which are taking place as a consequence of the injury. The condition will generally subside after a short time; and it is much better to allow it to do so, unless there is some special reason for interference.—It is well to suspend the scrotum, especially in elderly persons, so that it does not become moistened with the urine, to keep the parts as dry as possible, and to defend them by the occasional use of oil. I do not remember any instance in my practice in which the wound failed to heal soundly within a few weeks, or in which any unpleasant symptom, except slight difficulty in holding the water, followed; or any case in which the rectum was wounded, or in which any troublesome local symptom remained.—*Lancet*, June 1, 1872, p. 747.

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## 53.—ON LITHOTOMY AND LITHOTRITY.

By Dr. J. COOPER FORSTER, Surgeon, to Guy's Hospital  
and Lecturer on Surgery.

Do not use an old-fashioned instrument made somewhat after the style of what you know as a "prostatic catheter," but always take a sound with a very short beak. Here is one, for instance, which you will notice resembles very much the lithotrite in its form. With one of this kind you will find that in all children you will strike the stone very easily. In old people there may be some difficulty; but even if there be, it is rendered much less likely to occur with the instrument such as I have shown you. To take a case in point: it is by no means uncommon to have to sound a patient for stone who also has a large prostate; and, if a calculus be present, the chances are that it will be found in the pouch immediately behind that organ. It can then only be struck by turning the beak downward, so that the end may pass into the recess in which the stone lies. What would you do if you had an instrument with a large curve? Turn it downwards you could not, and hit the stone without probably you could not; while, if the unfitness of the instrument for the special case had not crossed your mind, it would probably be withdrawn without finding anything, and you would treat the patient for something else till he had grown tired of his non-improvement and had sought the advice of some abler man. Many a stone has been missed behind the prostate simply because the old instruments could not strike it in that position. This leads me on to another point, which is, that a stone may be struck at one time and not at another. You will have noticed that the child had been sounded six different times at another hospital, and the stone was not felt; and when we come to look at the stone, we see that it must have been in the bladder for a very long time. I take it that a stone of this size ( $\frac{7}{8}$  in.  $\times$   $\frac{7}{8}$   $\times$   $\frac{1}{2}$ ) has been forming much longer than four or five months, the period during which the symptoms have existed. Now it is absurd to suppose that in any hospital the surgeon does not know how to sound a child and detect a stone. It is a proceeding which does not require much art, and any of you would be able to do it easily enough after having passed a catheter a few times. If, then, this boy was sounded so many times and no stone detected, it could only be that for various reasons calculi, even of considerable size, cannot always be felt. The reason that it escaped notice in this instance I believe to be owing to a very closely contracted condition of the bladder, and which caused a little delay in extracting the stone at the time of the operation. This was not because the stone was small, and it was not because my incision was too limited,



for having once grasped the stone it came away easily ; but it was because the bladder was perfectly contracted, and contained no urine. If a sound is passed into the viscus in this condition, it will have no room to play, and very likely will become entangled in the folds of mucous membrane without once reaching the surface of the stone.

In addition to the stone from which this little boy suffered, I would ask you particularly to notice another feature. He has a prepuce which is adherent to the surface of the glans. This condition is important, because it is capable of producing many most marked symptoms of calculus. Indeed I think I would go so far as to say that in nine-tenths of the children brought to you for incontinence this symptom will be owing to the adhesion between the prepuce and glans. Knowing this, and seeing that the child had such a condition, it perhaps might have been as well to have separated the surface of these parts before proceeding to pass a sound. We should, however, have then had to wait to ascertain if the symptoms subsided. The boy, however, came under my notice as one in whom the presence of stone had been already settled, and therefore it was not necessary to wait ; but I shall take care that circumcision is performed before he leaves the hospital to ensure his freedom from trouble hereafter ; and I would advise you, whenever a case of suspected stone is brought to you, always to inspect the prepuce first, and if it be adherent, to separate it before you attempt to pass a sound.

In sounding adults whom in all probability you will, should you detect a stone, determine subsequently to lithotrise, you will find it a better plan not to do as I did in this case, for a special reason—viz., pass an ordinary sound first,—but pass a lithotrite at once. It is not more difficult of introduction, and you can detect a stone with it as easily as by a sound. It drops into the bladder almost by its own weight, and you can then, should a small stone be there, if there be no indication to the contrary, crush it at once. I have pursued this plan in private practice, and only the other day a gentleman came to my house with symptoms of calculus, for whom I passed the instrument, caught the stone, and crushed it at once. The stone being broken, he was, therefore, so far on his way to being relieved of his malady. I always, where possible, use the non-fenestrated blades, that the fragments may be more completely crushed, and for convenience I know of no instrument so perfect as Weiss's lithotrite, in which, by merely sliding backwards or forwards a button in the handle, the screw on the inner blade is at pleasure fixed or set perfectly free, to allow of a sliding motion on the outer blade.

I need hardly say much as to the choice of operations. You

know as well as I do, that in adults, if the stone be not too large, or the bladder symptoms too intense, crushing should always be undertaken, because it is much less fatal than the cutting operation. In children, on the other hand, lithotomy is so satisfactory that one could hardly improve upon it by any other method.

I have cut a good many cases in children now, and, with the exception of a single painful case in my earlier years, all have been uniformly successful. All men have some accidents in their less skilful days of commencing practice, and I do not believe in that man who never makes any mistake. Let us only be honest, and relate them, that you who are to succeed us may be fully alive to the mishaps which may occur, and which, unless you know that similar ones have occurred before, may, when they come to you, unduly weigh upon and harass you. In one case I tell you that I failed to extract the stone, and the child died, I believe in consequence of the operation.

After lithotrity, according to the number of times you may have crushed the stone at that sitting, there will be of course more or less detritus in the bladder. The passage of this may often be much facilitated by using this instrument invented by Mr. Clover. It is most ingenious, and at times very useful. It consists of a large catheter with a very full-sized eye, or completely open at the end, to which is attached at the upper part a glass chamber, and to this, again, an india-rubber flask. The catheter is introduced into the bladder, and the air having been expelled from the flask by filling it with water, it is attached to the catheter, and the water forced into the bladder by compressing the side of the flask. When allowed to expand again the water is sucked back into the flask, and any fragments that come with it subside to the bottom of the glass chamber. This process can be repeated as often as necessary.—*Lancet*, Oct. 26, 1872, p. 592.

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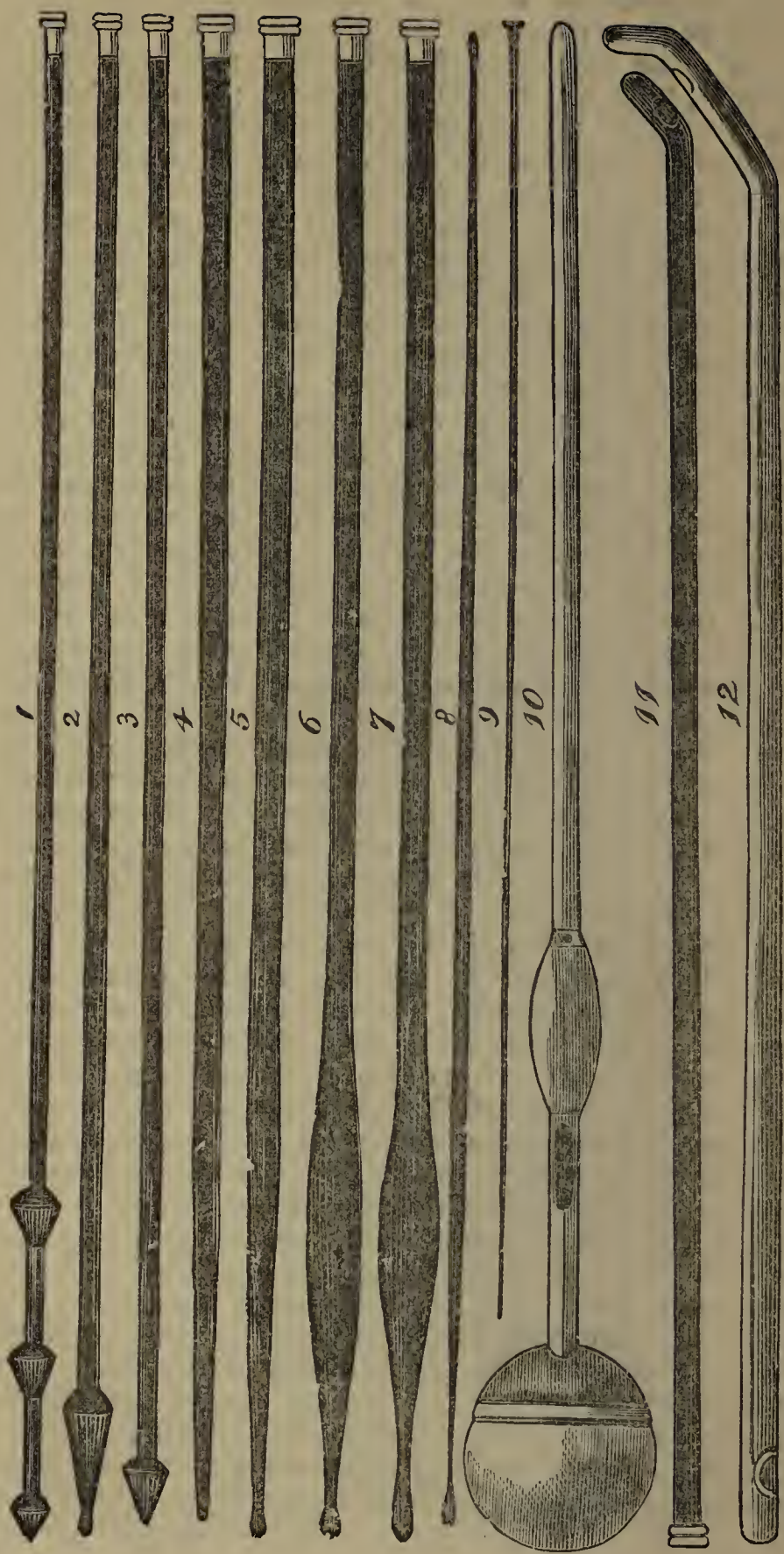
#### 54.—INSTRUMENTS FOR TREATING AFFECTIONS OF THE GENITO-URINARY ORGANS NOT COMMONLY KNOWN IN ENGLAND.

By W. F. TEEVAN, Esq., B.A., Surgeon to the West London  
and St. Peter's Hospitals.

Without some of the instruments I am about to describe no surgeon can diagnose and successfully treat many of the affections of the genito-urinary organs. Two of the instruments depicted have within the past few years found their way into English practice, and I have no doubt the others will before long become as familiar. I would premise my observations by saying that, except in certain particular cases, the employment



of metal instruments is to be deprecated as causing more annoyance to the patient and far greater local irritation than soft ones. The saying of M. Mercier ought always to be remembered, "La sonde rigide ne cede pas ; c'est l'obstacle qui cede devant elle." The use of a stiff, unyielding instrument in a passage often tortuous and rugged can only be accounted for by an ignorance of those pathological alterations which take place in stricture of the urethra. It is hardly fair to a patient for a surgeon to commence the practice of catheterism in the most sensitive tract in the body without previously ascertaining by a trial on his own person the comparative merits of soft and metal instruments, and I hope that before long the latter will be rarely seen. There are, however, some surgeons who will not be baffled in the passage of an instrument, and hence these gentlemen will always remain the staunch advocates of an order of things which, happily for humanity, is passing away. Armed with a pointed silver catheter no surgeon need be foiled in attempting to get into the bladder ; it is merely a question of force, more or less—no flesh can withstand its bloody onslaught. In one of the museums of London there is a very painful reminiscence of a celebrated surgeon, in the shape of a preparation exhibiting a metal instrument *in situ*, which he had managed to pass into the bladder *without traversing the prostatic urethra*. But I have heard it stated that some patients actually prefer metal instruments. Unfortunately, however, such a statement only shows that in a particular case the surgeon had, through an imperfect diagnosis, used an inappropriate instrument ; for if a patient with enlarged prostate and stricture—a common complication—be treated for the latter complaint with an olivary soft bougie he will certainly be subjected to considerable annoyance, for the end of the instrument will impinge against the enlarged lobe of the prostate, and can only be carried over the obstacle by sheer force, whereas a curved metallic bougie will only cause such inconvenience as is inseparable from the material of its construction. The progress of soft French instruments in England has been much retarded by the inferiority of those made here, for most of the catheters soon crack across the eye, and as a rule the olive is much too large, and the neck not being proportionately thick, the bougie gets bent at right angles at its neck, much to the patient's annoyance and the surgeon's fear, for I find there is a wholesome dread of the separation of the olive from its shaft. However with the Parisian instruments such an accident is impossible, and not only are they cheaper, but they are infinitely more durable. I consider a French bougie lives ten times longer than its English representative, which generally ends its short career by breaking its neck. For the intro-





duction of elastic instruments into surgical practice we are indebted to a French goldsmith, named Bernard, who brought them before the French Academy in 1779.

With the exception of Nos. 10 and 12 all the instruments I am about to describe are elastic.

Fig. 1 illustrates the "bougie à trois nœuds." The cone at the end is succeeded by others, each larger than its predecessor. The bougie may be used for dilating strictures or applying ointment to any part of the urethra. It is well known to those who use the bougie à boule that although at first the cone may only be able to get through a stricture, yet, if the instrument be moved to and fro a few times, the obstruction will no longer be felt, owing to the dilatation exercised; and it is the knowledge of this fact which enables us to dilate a stricture considerably at one sitting, if it be thought desirable, by the use of the "bougie à trois nœuds," for each cone, being larger than its predecessor, more and more dilates the contracted part. The instrument is peculiarly adapted to ring strictures, and I would say to them only.

Fig. 2 is the "bougie à boule" as altered by Dr. Mallez; and Fig. 3 is Dr. Henry Dick's modified "bougie à boule." The latter was described by me at p. 542 of vol. i. of the *Lancet* for 1870, and I simply introduce it here for comparison. Dr. Mallez claims for his instrument an easier introduction, on account of the tapering olive he has added to the bougie; but inasmuch as the ball-staff is for diagnosis only, I prefer Dr. Henry Dick's, as the greater sharpness of the base of its cone permits an increased perception in diagnosis of pathological alterations. These bougies are made of various sizes, but the most useful are those varying in size from a No. 6 to No. 10, English measurement, which enables us to detect incipient contractions long before they can be ascertained by any other instrument. For a knowledge of the value of these bougies I would refer the reader to an article by me in the *British and Foreign Medico-Chirurgical Review* for July, 1867.

Fig. 4 represents the "bougie conique." At p. 454, vol. v., of the *Dict. de Méd.*, M. Desormeaux in the following words states the value of these conical bougies: "Ces bougies sont, sans contredit, les plus utiles dans le traitement des rétrécissements, dans lesquels elles s'engagent et qu'elles dilatent mieux qu'aucune autre." I find from my own experience that these bougies are most useful in boring through, as it were, tight, narrow strictures, and enabling the case to be completed with the olivary bougies.

Fig. 5 illustrates the "bougie olivaire," already described by me in the *Lancet* at the part referred to. This bougie is applicable to the treatment of the majority of cases of stricture

which will admit a No. 4 or 5 English. Its great value is dependent on its olive, which prevents the point from lodging in the lacunæ of the urethra. It is also the only instrument which ought to be placed in a patient's hands; he cannot do any harm with it, the formation of a false passage being an impossibility even if the patient tried. As will be seen by the illustration, this bougie is the conical bougie *plus* the olive placed at its extremity. It was invented by Lioult at the commencement of the present century, and obtained a prize at the Academy in Paris.

Figs. 6 and 7 represent respectively the "bougie conique à ventre" and "bougie olivaire à ventre." When we have dilated a stricture up to a large size, say No. 12 English, the patient often complains of the unpleasant distension of the meatus externus, which has once more become, as it ought to be, the narrowest part of the entire canal. Now, inasmuch as the shaft of these bougies is much smaller than the other part, we shall, if we use them, only subject the patient to an annoyance lasting for one second, instead of the prolonged inconvenience resulting from having a full-sized ordinary bougie in the passage for several minutes. The instrument dilates the stricture only.

Fig. 8 is a representation of the olivary whalebone bougie. Each end is similar to the other, consequently it will last as long as two bougies not similarly constructed. All bougies might be made double, as it were, by uniting them at the end of the shafts. These bougies can be obtained with ends smaller than a No.  $\frac{1}{4}$  silver catheter, and they are of inestimable value, for they combine all the advantages of elastic instruments *plus* the special merit of stiff ones in those particular cases where the perineum is one solid mass of induration; the olive at the end prevents the formation of a false passage, which is quite possible if a tapering-pointed whalebone bougie be used. In cases of traumatic stricture these instruments are invaluable, as they are usually the only ones that can be got through.

Fig. 9 is the elastic filiform bougie, and is the smallest known; its point is necessarily conical, as its extreme tenuity will not admit an olive to be placed on it. Some idea of its size may be gained when I state that a No.  $\frac{1}{4}$  silver catheter is equal to about six of these bougies put together. Not only is it most useful for treating all those cases of stricture which are absolutely impassable to any English instrument, but it will also obviate recourse to an operation in cases of retention of urine from very bad strictures, for if the bougie be passed and left in for a few minutes, urine will be found to flow on its withdrawal. The surgeon who has had no experience of this instrument will very naturally suppose that it will double up



on meeting with an obstacle; but such will not happen if he takes his fingers off the bougie at every half inch of progress made, for if it cannot proceed further, it will spring back, thus giving the surgeon notice of its arrest. It must be wriggled in, rather than introduced in the ordinary manner.

Fig. 10 is Dr. Mallez's instrument for injecting powders into the urethra, as modified by me. I have made it a breech-loader, and so simplified its action. The instrument is useful for injecting various astringent powders into the anterior part of the urethra in cases of chronic urethritis.

I now come to two instruments which no surgeon ought to be without—M. Auguste Mercier's "sonde coudée" and "sonde bicoudée." The beaked catheter No. 11 resembles a lithotrite so far as its beak is concerned, which is placed at much the same angle to its shaft. This instrument is destined entirely to supplant the English metal prostatic catheter, and also the elastic catheter within a stylet; it never draws blood, and can be used either by the surgeon or patient in cases of retention of urine from enlargement of the third lobe of the prostate. But cases of difficulty often arise either from the extreme obesity of the patient or from the great extension of the floor of the prostatic urethra, and no ordinary prostatic instrument can be passed; for such cases M. Mercier's "sonde bicoudée" (Fig. 12) is invaluable. I believe it is practically unknown in this country.—*Lancet*, Aug. 24, 1872, p. 255.

#### 55.—A NEW METHOD OF TREATING HYDROCELE.

By S. MESSENGER BRADLEY, Esq., Manchester.

While the various plans of treating hydrocele hitherto recorded possess the prestige of a high antiquity, they all alike suffer from being occasionally unsuccessful, or even hurtful, in their results. These objections hold good, though in a less degree, in speaking of the treatment by tapping and injecting the vaginal sac, which has practically superseded all other modes. This operation, first recommended by Celsus, who advised nitre as the best injection, fell into a long desuetude after his death, until revived by Monro the elder, and of late years popularised by Sir Ranald Martin, whose claim to originality lies in his choice of iodine as the most suitable stimulating agent. Other plans are, however, resorted to from time to time, either from their greater safety and simplicity, or from the occasional failure of the iodine treatment. Thus, briefly to summarise these methods, we have—1, *treatment by acupuncture*, recommended by Lewis, and still sometimes adopted and found to succeed in cases of congenital hydrocele; 2, the mere *application of an evaporating lotion*, such as muriate of ammonia, vine-

gar and water, which, it is probable, has only been found of service by Keate, who, I believe, was the first to recommend it to the profession; 3, *simple tapping*, nearly always failing to effect a cure, and not always without danger, inasmuch as it is sometimes followed by a hæmatocele, or even sloughing of the scrotum; 4, *laying open the sac*, a plan approved by the fathers of medicine, but abandoned by their descendants of the present day; 5, *excision of a portion of the tunica vaginalis*, which has, in having been practised by Albucasis, an almost equal antiquity with the one last mentioned, and has met with quite an equal neglect; 6, the plan of *evacuating the fluid and introducing some caustic on the end of a probe*, of which Paulus Ægineta writes in warm praise, and which, though occasionally adopted, as Humphry states, at the present day, is not likely either from its success or safety, to become more general than it deserves; 7, *the introduction of a tent into an open wound*, as performed and praised by Paré, Baron Larrey, and others; and 8, the somewhat similar plan, still, I believe, commonly practised by the Arabians, who were the first to adopt it, of *passing a seton through the vaginal sac, and there retaining it for twenty-four hours*. It is likely enough that this operation would succeed in cases which resist all milder treatment, but, from the by no means trifling danger attending it, it should not be resorted to if we can equally achieve our object by a safer mode of procedure; and this, I believe, can be done, as I will endeavour to show.

It very frequently happens that a hydrocele must be treated, if treated at all, in the out-patient department of a hospital or at the surgeon's residence; that is to say, at a distance from the patient's own home. Now the disadvantages arising from this fact are, that the walk home after operation is apt to induce considerable and even dangerous inflammation, or that a hæmatocele ensues as the result, not necessarily of wounding the testicle, but of a dribbling from the scrotal veins, which are turgid from their dependent position.

Pondering these circumstances, and also reflecting upon the fact that the walls of pyogenic membranes, such as those of abscesses, sinuses, and the like, will often agglutinate when brought into firm and continued apposition; and remembering, at the same time, that the serous tunic of the testicle is from its physiological nature liable to take on adhesive action, and that, from the character of the secretion poured out in a hydrocele being inflammatory and not dropsical, it would even be prone to do so, I was led to the inference that simple tapping, followed by firm and equal strapping of the affected side, would probably be followed by an obliteration of the vaginal sac and a consequent radical cure.



It was not long before I was enabled to test the accuracy of this reasoning. A medical man applied to me with a large simple hydrocele, which had been tapped several times, and the last time injected with iodine without success. After explaining my object to him, I tapped the hydrocele, drawing off half a pint of fluid, and tightly strapped the affected testicle with soap-plaster. This was done at my own house, and the patient walked home, a distance of about a mile, immediately afterwards, and continued to go about during the process of recovery, which probably took place in about ten days; I say probably, as I kept up the pressure for three weeks without allowing the testicle at any time to remain unsupported. This case occurred eight months ago; since then I have followed the same course in three other instances, and in each with an equally ~~\*\*\*~~ satisfactory result. In no case was there any fresh effusion of fluid. Another case which came under my notice was of some interest in illustrating the advantages of strapping in what would beforehand appear quite unfavourable circumstances. A man came to consult me about a recent hydrocele of some magnitude; I tapped and emptied the tumour, but did not strap it at the time, as there was a strong force of pediculi encamped in the pubic and scrotal hair; ten days afterwards, he visited me again, having got rid of his unwelcome guests, but with his tunica vaginalis as much distended as ever. I again tapped him; but, though I do not think I wounded the testicle, which could be plainly enough seen at the back of the tumour, I did not succeed in drawing off any fluid worth speaking of; nothing followed, indeed, but a few drops of bloody serum. In three days he came again with his scrotum larger than ever. The tumour had now, however, changed its character; it was now no longer transparent and pear-shaped, but opaque and rounded; it had also become very heavy and much more painful than it had ever been before. In other words, a hæmatocele had formed. Without the anticipation of much good resulting, I resolved to try the effect of strapping in this case; suffice it to say that this proved effectual, not only in causing the absorption and dispersion of the vascular extravasation, but also in permanently curing the hydrocele. In spite, however, of the success in this instance, I am not inclined to think that the plan would prove generally efficacious in the treatment of even recent hæmatocele, and I do not now at all desire to advocate it in such cases.

In regard, however, to hydrocele, it appears to me that we have in this plan of tapping and strapping one which satisfactorily fulfils the idea of curing safely, quickly, and pleasantly, and which, though perhaps not about to prove infallible, is one which should be certainly tried in all cases (especially, I would

add, those treated away from the patient's home), before the injection of iodine or other stimulant is resorted to. If cases occur in which neither the mode I here advocate nor the iodine treatment is successful, I am of opinion that a combination of the two would be likely to prove so.—*British Medical Journal*, June 1, 1872, p. 580.

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#### 56.—VARICOCELE.

[The following is from a report of operations at St. Thomas's Hospital.]

Mr. Sydney Jones operated in a well marked case of this disease, as usual, on the left side. The testis was atrophied, and the patient suffered from much aching pain. Mr. Jones proceeded in the same way as he adopts for ordinary varicose veins of the legs, and which he has invariably found to succeed. The vas deferens being held out of harm's way, two harelip-pins were passed behind the veins, and thick silk used so as to compress the opposed sides of the veins, but not so tightly as to cause ulceration; a tenotomy-knife was then introduced so as to divide subcutaneously the veins between the two pins. The patient was then transferred to bed, and an ice-bag applied. Mr. Jones stated that the pins were allowed to remain from seven to fourteen days; the time varying according to the amount of irritation produced.—*Medical Times and Gazette*, Sept. 21, 1872, p. 326.

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#### AFFECTIONS OF THE EYE AND EAR.

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#### 57.—THE TREATMENT OF CASES OF STRUMOUS OPHTHALMIA.

By Dr. HENRY POWER, Senior Ophthalmic Surgeon to St. Bartholomew's Hospital.

[Belladonna taken internally exerts a remarkable influence over the progress of strumous ophthalmia, in certain cases when other plans have failed. After describing the well-known general aspect of a case of strumous ophthalmia, the writer continues:]

The group of symptoms that are then presented are that, with the general aspect above described, the child exhibits intense intolerance of light; it holds the head down, covers the eye with the hands, and resists strenuously their removal; the least attempt to open the eyes causes pain, copious flow of tears, and not unfrequently sneezing, when a large quantity of muco-purulent fluid escapes from the nostrils; the lids are



often almost natural, or but slightly red and swollen. To obtain a view of the cornea is no easy matter even to a practised hand, in consequence of the spasmodic action of the corrugator supercilii and orbicularis palpebrarum; and if it cannot be accomplished by placing the child with its back to the light and coaxing, nothing remains but to take a seat opposite the nurse, receive the head of the child between the knees, and raise the lid with a speculum. This proceeding, though perfectly harmless, is often regarded by the friends as a piece of refined cruelty, and it is perhaps better to administer chloroform, which has itself and alone sometimes appeared to me to produce a marked change for the better. It is advisable to make one thorough examination in the first instance, as occasionally ulcers of considerable depth may be found, which, unless properly treated, may perforate and leave an unsightly scar and great impairment of vision. Yet, in the majority of cases, the appearances of disease presented when the eye is fully exposed are wonderfully slight. A little redness of the conjunctiva, a small ulcer or two near the margin or on the surface of the cornea, sometimes with a leash of vessels running up to it, a superficial abrasion of the corneal epithelium, and that is all. The cornea is elsewhere clear, the conjunctiva and sclerotic almost of their natural colour, the iris apparently healthy, with a much contracted pupil.

The cause of the intense photophobia, so out of proportion to the local inflammation, has occasioned much speculation. It is probably to be attributed to an exaltation of the sensibility of the optic nerve, resulting from irritation of the terminal branches of the corneal nerves. These branches we now know, from the researches of Klein, pass up between the cells forming the corneal epithelium, nearly, if not quite, to the surface, where it is probable they end by forming a plexus. When the epithelium is abraded, or, owing to defective nutrition, is not properly regenerated, the nerves must be exposed, and every motion of the lid must irritate them. Such irritation is believed to act reflectorially on the optic nerve-centres, producing photophobia, and also on the branches of the fifth, supplying the lachrymal gland, causing the profuse lachrymation.

The first stage in the development of one of the little herpetic ulcers, as they are often called, appears to be, as Iwanoff has shown, a minute collection of nuclei or nucleate cells between the epithelium and the anterior layer of the cornea. These appear to be a proliferation of the corpuscles of the sheath of the nerves, or at least to travel along the nerves between the sheath of Schwann and the nerve substance, and thus form a pale vesicular-like swelling, which soon bursts, and leaves a small ulcer with yellow base, if on the conjunctiva, or with a

whitish surface, and surrounded with an area or halo of grey, troubled or cloudy aspect, if on the cornea. In this state it may last for many days, neither materially increasing nor exhibiting any tendency to heal. In some bad cases, however, especially if the child's bowels and general health have been much neglected, it rapidly extends, and, perforating the cornea, leads to prolapse of the iris, after which the whole affection quiets down.

Wherever a disease is of a constitutional nature, and requires general treatment, directed by an intelligent appreciation of its etiology and of all the circumstances leading to its persistence, the remedies employed must necessarily be various, because each case requires to be treated upon its own merits, and the sound practitioner finds that he must vary his treatment in accordance with its exigencies; and also because there have always been those who, mistaking the *post hoc* for the *propter hoc*, have attributed to the action of some drug or agent acting locally, which has really been due to other circumstances acting generally. Hence alike the origin of and the disappointment occasioned by specifics. It appears to me that in the treatment of this disease, attention should always in the first instance be directed to the state of the bowels. It is advisable to see the motions, for valuable indications of treatment may be derived from them. As a rule a brisk purgative is very useful at the outset, and I generally prescribe five or ten grains of the compound jalap and scammony powder, with one or two grains of calomel, which, if the scammony be of good quality, acts well. An emetic repeated for two or three mornings, as suggested by Mackenzie, sometimes acts capitally. Ten or twenty grains of ipecacuanha may be given with half a grain or a grain of potassio-tartrate of antimony, according to age and strength. When the bowels are quite natural, some form of tonic is required, and our choice usually lies between the preparations of iron, quinine, and cod-liver oil. The former may be given in rather large doses, as from ten to fifteen minims, the quinine in one or two grain doses, and the oil in one to four drachm doses, three times a day. The latter remedy acts best in children of fair complexion. Great advantage will be found in adding two or three grains of hydrargyrum c. cretâ to the quinine. For some years I always commenced the treatment by giving this combination to children suffering from strumous ophthalmia, using no applications to the eye except atropine, and was well satisfied with the results.

General treatment, however, is by no means sufficient to cure the patient, and amongst the many local remedies I have tried, I should give the palm to atropine, in a two or four grain solution, Pagenstecher's yellow ointment, and calomel. With one or other of these most cases may be cured.



Cases, however, occasionally occur in which all these plans of treatment fail; and the question comes, what must now be tried? It is then that I claim attention to the value of extract of belladonna, given internally. I have repeatedly found that it rapidly diminishes the intolerance of light, and by its power of relieving the spasm of the muscles closing the lid, enables the child to obtain an amount of benefit from air and exercise that was previously impossible. I can entertain no doubt that its good effects are attributable to its action as a stimulant upon the sympathetic system of nerves, and through this upon the smaller vessels. It is further of use in doing away with the necessity of purgatives, as even in small quantities it acts efficiently in clearing the bowels. I have usually prescribed it in doses of one-eighth to one-quarter of a grain. It is perhaps scarcely necessary to add that, as it is a potent remedy, its effects must be watched, and its administration should be suspended as soon as the child complains of thirst, or when the rapidity of the pulse is observed to be increasing. I have only noticed these symptoms in one or two instances. I have also found the extract of belladonna serviceable in cases where the affection was rather a limited keratitis than phlyctenular ophthalmia; that is, in which a small segment of the cornea was hazy and vascular near the margin, even though the tolerance of light may not have been very intense. I consider the seton, though recommended by so good an authority as Mr. Bader, a *pis aller*; and find the treatment mentioned by Dr. Swanzy, as practised by Von Gräfe, though it was originally suggested by Jüngken, of dipping the whole head for a few seconds in cold water, not persistent in its effects.

The following is a *résumé* of one of the cases that has recently been under my care, showing the benefit that may be derived from the use of the extract of belladonna in obstinate phlyctenular ophthalmia:—

M. C., æt. four, was brought to me in the beginning of January of the present year. She had had measles (not an unusually severe attack) in August 1871; this was followed by stoppage in the nostrils and herpetic eruption on the lip and on the right ear. At the end of August her mother took her to Yarmouth for a month. She was ailing all the time she stayed there, complaining of thirst, with restlessness at night, languor, and loss of appetite. These symptoms continued up to November, when the right eye became affected. She was taken to an ophthalmic hospital, when some tonic medicine was ordered for her, and a poultice was directed to be applied. A crop of pustules appeared over the brow, temple, and cheek, and the left eye became affected. I then saw her, and found that she was suffering from a well-marked attack of phlyctenular ophthalmia.

mia. On examining the eyes several pustules were visible on the conjunctiva at the margin of each cornea, whilst superficial abrasions or ulcers were apparent on the corneæ themselves, the right being much the more severely affected, so that a large part of its surface was cloudy. She was ordered a purge, and a powder containing three grains of hydrargyrum c. cretâ with one of quinine, thrice daily. A four-grain solution of atropine was dropped in, and a lotion of extract of belladonna given to take home, and to be used frequently. But little improvement resulted, though the mercury and quinine were continued for a fortnight. The bowels being somewhat disordered, and the lips and ala nasi having some pustules upon them, it was thought likely that there were worms. Another purge of compound jalap powder and a turpentine injection were therefore ordered. No worms, however, were observed in the motions; cod-liver oil was now prescribed, and after a short time a little steel wine was added to it. The use of the belladonna lotion was continued. This plan was persevered in for a month with little benefit; the child sometimes opening the eyes in the evening and playing about, but always coming to me in the morning with intense photophobia. The mother was exceedingly intelligent, and assured me that the bowels were regular, the motions natural, and that the food given to the child was carefully selected, wholesome in quality, and though its appetite was variable, yet upon the whole the quantity was sufficient for its needs. Calomel powder was now applied directly to the cornea for several days, but ineffectually, so far as the relief of the photophobia was concerned.

Finding that the symptoms were stationery, I now determined to tap both eyes with a broad needle, and allow the aqueous humour to escape. The benefit was considerable, the child opening the eyes on the following days for some hours, and I thought recovery would take place without further trouble. However, a relapse soon took place, and she was now placed upon one-sixth of a grain of good extract of belladonna three times a day. In two days the mother brought her to me obviously greatly improved. The intolerance of light had greatly diminished, and she even saw to pick up a pin on the carpet. She continued the use of the extract for a fortnight, when it was given up, and some quinine mixture ordered. A few days later she went into the country. This child has lately been seen by me, looking fresh and rosy, with only slight nebulae on the corneæ of both eyes, but able to bear strong light perfectly; and I can feel no hesitation in attributing the improvement here observed to the internal use of the belladonna, it was so immediate and proved so persistent.—*Practitioner*, October 1872, p. 203.



## 58.—ON THE MODERN ART OF TINTING OPACITIES OF THE CORNEA.

By Dr. CHARLES BELL TAYLOR, Nottingham.

Extensive leucomata and dense opacities of the cornea have long been considered among the opprobria of surgery; we could not remove them, they were always a source of embarrassment to the surgeon, and, especially in the case of women and young persons, a serious trouble to the patient, so much so that I am convinced that patients suffering from this cosmetic defect and applying for relief cared more for the removal of the deformity than for the restoration of the partially intercepted sight. Such being the case, we hail with gratitude, as a most important addition to modern ophthalmic surgery, M. Wecker's reintroduction and perfection of an ancient method of tinting away these unsightly defects.

The operation, which, as a rule, causes very little pain or irritation, is best performed with a number of the finest needles firmly bound with the points on a level around a handle such as a penholder, or a large needle which has been grooved for the purpose by Messrs. Weiss may be substituted with advantage in certain cases. The substance which M. Wecker recommends for tinting is Indian ink; but I have also employed sepia, ultramarine, and other colours with advantage, and when an immediate and deeply coloured effect has been desirable, a combination of lamp-black with Indian ink, and a solution of nitrate of silver. The patient may either recline or be seated in a chair, and it is well to separate the lids with a speculum and steady the globe with a pair of ordinary forceps, taking a firm grasp of the conjunctiva. The needles are then dipped in one or other of the solutions in question, which should be made as thick as possible, and the superficial layers of the cicatrix are rapidly punctured in an oblique direction, and layers of the solution applied just as in ordinary tattooing, until the white speck is changed from a most apparent deformity into a black surface scarcely visible; a fresh layer of the substance is then applied over the tattooed cicatrix, the patient is directed to keep his eyes open, so as to let it dry on and remain as long as possible, and he may at once go about his usual avocations.

It is important not to close the eye, and to prevent as far as possible, the washing away of the pigment by the tears; this is best accomplished by enveloping the operator's fingers with a silk handkerchief, so as to mop up the secretion, and afterwards by the avoidance of winking on the part of the patient.

M. Wecker is content with ten or fifteen punctures a time, and requires from his patients four or five sittings. I have, however, usually completed the operation at once, and made any little addition that might appear necessary some weeks later; the slightest specks, such as are left after phlyctenular conjunctivitis and small ulcers of the cornea, are easily banished by one or two pricks of the needle: and when the whole eye has been opaque, very unsightly, and sight completely abolished, I have very advantageously substituted extirpation of the globe, or the superimposition of an artificial eye, by tatooing a round central pupil so as to restore to a remarkable and most charming degree the natural appearance of the globe. Not only is deformity removed by this slight operation, but patients generally tell us their sight is improved, a fact due, no doubt, to the circumstance that a black speck is much less dazzling than a white one.

In cases of large cicatrices, I generally make an artificial pupil first, and tint the opacity afterwards; and when this is carefully done, it is difficult to distinguish the black speck from the neighbouring artificial pupil. I have no doubt that this little operation will also be found of service in cases where considerable dazzling follows the removal of a portion of iris, either *per se*, or when the operation has constituted a part of the operation of extraction for cataract; as by tatooing the cornea an invisible opacity may be occasioned which will constitute a permanent shade, and shut out the light to any required extent; soft cicatrices are readily coloured, but old, hard, and incrustated ones are more difficult of treatment. In these cases, the nitrate of silver and lamp-black are of service; no doubt, also, lead, sulphur, charcoal, gunpowder, and other ingredients will come to be employed in time. In some cases, the coloration is not very permanent, but it may always be repeated; and even if the perfect blackness do not remain, a greyish semitransparent coloration takes its place, which looks very like cornea, and it is infinitely preferable to the original deformity.

It is well to commence the tatoo at the lowest surface to be operated on, in order that the operator's sight may not be obscured by an overflow of the liquid. I shall conclude by quoting a case in which this addition to our armamentarium enabled me to render a real service to a young lady who was suffering not only from loss of sight, but also from a most embarrassing deformity.—*British Medical Journal*, Sept. 7, 1872, p. 271.



## 59.—A NEW METHOD OF DESTROYING FAULTY CILIA IN CASES OF LIMITED TRICHIASIS OR DISTRICHIASIS.

By PATRICK J. HAYES, Esq., Surgeon to the Mater Misericordiæ Hospital, Dublin.

[For the information of those of our readers who are not well up in all the hard words coined by ophthalmologists we give Mr. Wharton Jones's definition of districhiasis. It is "merely a variety of trichiasis, in which the misdirected eyelashes are disposed, though not very regularly, in a row distinct from the other which remain properly directed." Mr. Hayes relates two cases in each of which the affection had been of long duration, and operative measures employed which had produced in both instances a certain degree of permanent corneal opacity, with impaired visual power.]

*Case 1.*—This patient was a young countryman, of strong, though rather strumous aspect. For several years his eyelids and eyes had been weak and liable to frequent attacks of inflammation. On examination I found he had opacities of the right cornea, resulting from old ulcerations; also a slight amount of symblepharon and blepharo-phymosis; but the latter conditions did not produce any real inconvenience. The left eye was somewhat irritated in consequence of the backward direction of cilia growing near the outer part of the upper eyelid, for though the patient had pulled out several large hairs, yet some fine and short ones often escaped the grasp of his forceps.

I informed the man that by a slight operation I believed the irritating eyelashes might be removed, but that nothing could be done to improve the condition of the right eye. This opinion afforded him some consolation, and he at once consented to the treatment proposed. I desired that in the meantime he should not remove any of the growing hairs, as they would guide me in adopting measures for their extirpation. Deeming the case a suitable one for the operation proposed by Herzenstein, I passed a subcutaneous ligature from the palpebral margin, so as to enclose the roots of the offending cilia; but the result was, to a certain extent, incomplete, partly because, in spite of my directions, some lashes had been extracted, and, therefore, the position of their follicles could not be ascertained when the ligature was being passed, but also an irregular line of very small hairs existed so close to the posterior, or conjunctival edge of the free margin of the lid as to render their obliteration by Herzenstein's method quite impossible. I now determined to employ subcutaneous injection of tincture of the perchloride of iron, which I knew from experience might be so used as to destroy the vitality of a very limited amount of tissue. I per-

formed the operation in the following manner:—A Desmarres' entropion forceps was applied to the eyelid, and so compressed as to cause arrest of circulation through the included portion. Next, a fine hypodermic syringe, charged with a few drops of the tincture of perchloride of iron, was passed obliquely through the palpebral margin, pushed to a point just above the roots of the abnormal cilia, and the tincture slowly injected, as it were, against the follicles. This proceeding was repeated in such a way that the two oblique punctures included the group of cilia to be destroyed. After a few minutes the forceps was loosened, the lashes carefully pulled out, and the eye bathed with cold water. The patient suffered no inconvenience from the operation until the third day, when inflammatory swelling of the lid appeared. This was treated with bread and water poultices, until ulceration of the palpebral margin permitted the escape of a narrow slough of subcutaneous tissue, with complete removal of the hair follicles. The patient soon left hospital, expressing great satisfaction, and stating he never had expected to feel his eye so strong and free from all irritation.

*Case 2.*—My second patient was a young woman who had been previously under the care of a distinguished surgeon in one of the principal hospitals in this city. According to her statement she suffered for a considerable time from trichiasis affecting the lids of both eyes, which produced chronic conjunctival inflammation and superficial corneal opacities. About eighteen months before her admission to the Mater Misericordiæ Hospital she consulted the surgeon already alluded to, who most skilfully performed Arlts's operation; but although the patient consequently derived great ease and benefit, yet a few cilia, forming a limited districhiasis, continued to cause considerable annoyance to the right eye. As the other patient was at the time in hospital, and afforded most satisfactory evidence in favour of the injection treatment, I adopted a similar method in this case, and with like permanent relief.

The chief points demanding attention for the safe injection of tincture of the perchloride of iron are:—Firstly, That the eyelid should be firmly compressed between the blades of a suitable entropion forceps. Secondly, That the fine tubular needle of the hypodermic syringe should be passed, and applied, as closely as possible, to the deep ends of the hair follicles. And lastly, That only a very small quantity of the tincture should be introduced.

If those rules be adhered to the operation affords a simple, comparatively painless, and effective means for producing the required extent of madarosis.—*Dublin Journal of Medical Science*, March, 1872, p. 190.



## 60.—ON THE PRESERVATION OR DESTRUCTION OF THE CONJUNCTIVA IN CASES OF EXTIRPATION OF THE GLOBE.

By J. F. STREATFIELD, Esq.

When surgeons, not so very many years ago, used, ignorantly, very rarely to remove an eyeball, and never, probably, unless the case was supposed to be one of cancer, whether invading or not the surrounding orbital tissues, they took a long sharp-pointed knife, curved on the flat, and with a circular cut, including the eye, they removed it and a large portion of the neighbouring soft parts: the conjunctiva, in the general confusion, was, for the most part, and irregularly, also excised. But when it was found, by the injurious effects of sympathetic irritation and inflammation, that very many lost eyes (certainly all that are blind, in the ophthalmic sense, and painful) should be removed, and also that seldom comparatively any more than just the eyeball itself need be removed, and that when, and though, it must be removed, as little as possible need be done, a mode of operating for this purpose was re-introduced, sometimes now, therefore, called "enucleation": the conjunctiva was henceforth all and entirely to be carefully preserved, not only for the sake of its preservation, and that only as much as need be should be done in the operation, but to give a regular space, as large as possible, and sufficient room for the wearing of an artificial eye of good size. A considerable cavity, called the "conjunctival sac," is left thus after the operation, now usually done as I have said. In the centre, at the bottom of this space, is to be felt a nodule with a cruciform indentation, where the four rectus tendons are lying involved in the central dense cicatricial tissue, and are seen to move it about in some degree when the other eye is moved. And this hard mass is to support the artificial eye, and by the muscles of the excised eye it is to get all the movement that can be now obtained for an artificial eye. And this is the best we can do; for "abscission" is an operation that must be avoided, and will be certainly soon altogether given up. All this I believe to be indisputably true, and the best line of practice of which we have at present any certain intimation in a general way. It holds good in our private practice in nearly all cases, because the patients will almost invariably desire, as they can well afford the expense and the trouble involved in, the wearing of an artificial eye. But in cases of some few of these well-to-do patients of a different mind, and in very many of those of our hospital patients, this trouble and expense, if well understood beforehand by the patients themselves, will not be entertained, or, if it is entertained, it is in the future found to be impracticable.

Some patients, again, are met with, whether in private or in hospital practice, who either before or since the excision operation are quite incapable, and cannot by any surgical method be made capable, of wearing an artificial eye, even if they desire it; such are the cases of irregular cicatrisation within the lids, caused by wounds or burns, or caustics, or ill-surgery, and producing obstinate entropion, strong cicatricial bands in any direction (that cannot be isolated by a probe passed beneath them), or a narrowing of the whole conjunctival sac or of the palpebral aperture by any of the above causes. Once again, though the palpebral aperture be not narrowed nor the conjunctival sac contracted, there may be an obstinate ectropion, probably of the lower lid, which cannot be so well remedied by any surgical method as securely to retain an artificial eye.

I beg to propose now, instead of the unvarying rule of the preservation in all cases of the whole or as much as possible of the conjunctiva whenever the operation of extirpation of the globe is or has been done, its total destruction in the following cases.

1st. When the patient entertains an objection to the having to wear a glass eye, either before the excision operation or at any subsequent time, because of the trouble it occasions, the recurring expense, the annoyance of conjunctival discharges, &c.; and therefore he, after a full explanation, gives up the idea of wearing an artificial eye for the future.

2nd. When the wearing of an artificial eye is impracticable, and cannot be made practicable by the arts of surgery, by even any one firm band whose anterior end is at or near the edge of either eyelid. A full explanation, again, of the nature of the case to the patient is here also necessary; or, at some time hereafter, though the mind of the operator is free of any blame, yet, if he adopt the operation I now propose, the patient may falsely say *he* made it impossible that an artificial eye should be worn.

3rd. When the bands are many or large, or the contraction of the conjunctival sac is such, or the lids so considerably though incompletely bound together, or so everted, that neither the surgeon nor patient can have any hope of an artificial eye being worn.

At present the surgeon in view of such a case thinks he has no more he can do, and, indeed, he thinks the same in cases I have classed under the first and second heads. He dismisses the cases of all three classes from his mind as of those with which he has no longer any active practical concern. But I believe he may yet relieve the patient of much annoyance and of some of the deformity, by finally curing all discharges by altogether permanently closing the eyelids. At present the



patients of the three classes above are condemned for the future to go about with a small patch tied over the empty orbit, obliquely over the forehead and round the head. We constantly meet with them in the streets, and, if they are begging, they expose the hideous gap, bleared and running with matter by lodgment of foreign bodies in the sac, by the effect of changes of temperature, &c., on the sensitive mucous membrane, and, above all, by the irritation probably of the row or rows of eyelashes fallen in upon and brushing the mucous membrane, unsupported by an artificial eye. It is an improvement in *appearance*, as well as a comfort to the patient; in any case, if an artificial eye is not or cannot be worn, whether the palpebral aperture and conjunctival sac be full-sized or almost closed by cicatricial contraction. It is a disgrace to surgery not to close up the ugly chasm, and it is easily done.

I remember some time ago a man at University College Hospital for whom it was necessary that I should recommend a lost eye to be removed; hesitating and procrastinating, as patients in such cases, and not suffering much pain, will naturally do, finally consenting, and stipulating that the eyelids should be closed up over the wound, for he would not be troubled to wear an artificial eye. At this hospital there is a fund out of which any after-surgical appliance necessary for the good result of any operation can, on the certificate of the surgeon, be supplied. At the Moorfields Ophthalmic Hospital it is an old regulation that every patient for whom an eye has been removed, be supplied by the hospital authorities with a first artificial eye, and this regulation is well known to the patients and is constantly quoted by us in our endeavours to overcome the natural objections of patients to the removal of a lost eye; so that at this hospital our patients the sooner determine to have the necessary operation done, and accept, as a matter of course, the gift of the hospital. They think little of the saving of money that will be required of them to replace the artificial eye, which is, perhaps, soon broken, or is, in a year and a half or two years, worn out, as a remote event, and numbers of them come back to us sooner or later in one or the other case, lamenting their inability to procure for themselves the missing artificial organ. Then the money required at once is not forthcoming, and, in the absence of any competition in London in the making of the eyes to be worn, the expense is the more considerable. They wear the inevitable ugly patch in the empty orbit, or a handkerchief is tied over it, and, if they cannot afford an artificial eye, and it cannot be afforded for them, how much better off they would be with their two eyelids finally and totally closed together. I have lately at Moorfields had a patient who had had an eye either carelessly excised or previously injured in

such a manner as to have ultimately necessitated its excision, and a hard, firm, broad band of cicatrisation as a consequence extended back from a part of the margin of the lower lid into the deeper parts of the orbit. It was impossible to remedy this state of things and to enable the patient to wear a glass eye. He was much disappointed in this, but, as it could not be, he assented to my proposal that the eyelids should be entirely closed up. Under anæsthetic influence I incised the conjunctiva and dissected off great part of it, beginning at the bottom of the irregular contracted sulcus between the lids. The bleeding of course was excessive, and not all of the conjunctiva was removed. When healing of the wound thus caused had taken place, of course the lids were almost closed, but little deep pits and irregular spaces between the lids remained here and there, where some conjunctiva had been left. I then finally destroyed all this remaining conjunctiva by use of the actual cautery, the iron, red hot, thrust to the bottom of the deep hollows, and drawn horizontally along the parts between the lids yet lined with conjunctiva, so as to draw them closely together in the next process of cicatrisation. It was perfectly successful, and the patient can have no more unpleasant conjunctival discharges from useless mucous membrane, and his deformity is much ameliorated, as much as it is possible.

But it is so difficult to dissect off the delicate conjunctiva from the irregular surface and deep-seated space in which it lies, when the eye has been removed at a previous operation, and the bleeding meanwhile is so great as to obscure very much the operation, that I would suggest it to be destroyed by smearing over the conjunctiva, carefully preventing its transgressing the margins of the lids, a thin paste of chloride of zinc, to be left there long enough to destroy the conjunctiva, and then to be all thoroughly washed away, and the opposite surfaces allowed to heal. If any small spaces are then left between the lids of remaining conjunctiva they should be got rid of and closed up by use of a hot iron, and left to unite.

It will be so difficult to strip off or dissect away the whole conjunctival surface, leaving the subconjunctival tissues, at the first operation, when an excision is done, that it will probably be found most convenient to excise the eye, as it has been done of late years, preserving the conjunctiva and allowing the deep wound to heal; when, if the patient prove to be indifferent to the appearance of having a second eye, or unwilling or unable to submit to the expense or the trouble of the substitute, at a second sitting the conjunctiva may be then, with the assent of the patient, by a secondary operation totally destroyed by producing a slough of all the mucous surface, and consequently a final closing of the eyelids.



Besides these cases, it is, I believe, a surgical duty in all cases of incurable contracted cicatrices within the lids, or of thickened everted lids, that, when the eye has been removed, the useless conjunctiva be not left to produce offensive discharges, and to disfigure the patient more than he need be disfigured, and that a similar operation with chloride-of-zinc paste supplemented with the hot iron be adopted, or that some other means to the same end be employed.—*Lancet*, June 15, 1872, p. 821.

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61.—WHEN AND WHERE TO IRIDECTOMISE IN CASES OF ANTERIOR SYNECHIÆ REQUIRING AN ARTIFICIAL PUPIL OPERATION.

By J. F. STREATFEILD, Esq

In the practical suggestions I have put forth on this subject I have omitted to make a note of exception which had long ago been placed in the memoranda of my note-book, concerning the detachment of anterior synechiæ in iridectomy. If the iridectomy be done at an early period, when the inflammation has subsided and the adhesions in front are quite recent, the iridectomy may be done as well or better at any part of the circumference of the cornea (downwards and inwards?) that may seem to be best, without any regard to the corneal iris adhesions, which will freely separate themselves and cannot re-form subsequently. In an ordinary long-deferred iridectomy, where there are anterior synechiæ, and the iris is to be removed at a part where these exist, it is torn through its substance, and the part external to the tear is drawn out and excised. It probably bleeds very much, and the more central portion of the iris at the part chosen for the excision is left. Then there are greater probabilities of continuous bleeding from the iris or of a fresh inflammation of it. When, in these cases of adhesions of the iris in front, the operation is done early, and, if necessary, at the part where the adhesions are, the concave pupillary margin reappears in proper form, when and where the traction of the iris forceps is applied, and the iridectomy can extend up to this inner circumference, and an entire segment be removed and some at least of the hateful iris-adhesions are abolished.

But in the proper class of cases for these early iridectomies at any part of the circle—a class which is neither the commonest nor very uncommon.—I would not be misunderstood to mean that all cases in which anterior synechiæ form may be taken in this class and be thus successfully treated by operation. There are some few in which, though operated on early, the iris will only tear from the cornea; and the position of these

adhesions in these cases cannot be disregarded, but must be considered beforehand, as they have been by operators in the old way of deferred iridectomies for anterior synechiæ; indeed, when the cases can be diagnosed before the operation is begun, they may be deferred as all have been hitherto.

Where leucoma and anterior synechiæ follow penetrating wounds or perforating ulcers of the cornea not quite in the centre, and the aqueous humour being let out, the iris of course falls forward, and not only by apposition then with the back of the cornea becomes glued there, but, being opposite the external opening, it prolapses and becomes impacted or strangulated, as in old iridodesis operations, the iris will then only tear, and cannot detach itself in an iridectomy, which may therefore be deferred. Sometimes these cases are certainly to be diagnosed by the appearance in the white leucoma of a small dark-brown patch of uvea there fixed. The iris cannot then be separated as a whole. If we were to suppose a window-curtain to be in part accidentally glued to the shutters, separation might be effected without tearing by drawing the curtain aside; but if also shut in between the closed shutters, the tearing of the curtain must inevitably attend the act of separation. The simile is not perfect, or we must suppose the curtain affixed to the shutter would separate as a perfect whole only before the glue was dry.—*Lancet*, July 13, 1872, p. 38.

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## 62.—ON THE ADVANTAGES OF USING TWO SHARP HOOKS WHEN IT IS NECESSARY TO EMPLOY TRACTION IN CATARACT OPERATIONS.

By J. F. STREATFEILD, Esq.

There seems to me to be an increasing number of examples of the surgical instances in which it is found that two similar instruments are better in use than one. If an operator had many hands, instead of only two, it would be, no doubt, of great advantage to him. Some monkeys seem to have, practically, four (or five); but if we want to employ more than two hands we must engage an assistant for a third (or third and fourth) hand, and to him is entrusted the least important parts of the work to be done. Much more may be accomplished by determination and practice with our two hands than is usually done; and an assistant is, at best, a necessary evil in an operation, if he is indispensable. It does not at all interfere with the small movements required of our fingers in an eye-operation if the various little instruments are placed one in each of the inter-spaces of the first phalanges of the fingers of the left (or right) hand; three at least can be so held, besides the forceps to steady



the eye, which is committed to the thumb and forefinger of the same hand; the knife in the right (or left) hand. When each instrument has been used it is laid down, and we have not to wait an instant for the next that is wanted, or to arrange to hold it differently from the way in which it was given into our hand by the assistant. For my part, I frequently, in order to be independent, hold in my right (or left) hand, as well as in the other, more than one instrument, of those that are not sharp or cutting, between the palm of the hand and the ring and little fingers of the same. These remarks and suggestions would, I presume, hardly ever apply in general surgery, with larger instruments and more considerable movements of the hands and fingers in the operations. In eye-operations it is almost always best to be behind the head of the patient, lying on a high couch, with his feet in a well-lit window; and in operating, as in an ophthalmoscopic examination, it is best to stand, for then our movements are more freely made, not only from side to side, but by raising or lowering the head, so that the eye of the patient may be viewed in all directions and in various lights, as may be necessary, and this without approaching to or receding from the subject of the operation. Most eye surgeons prefer to sit down when operating or in an ophthalmoscopic examination; and for this purpose, in operating theatres and ophthalmoscopic rooms, at the hospitals at least, music-stools are provided: in operations, by way of getting, in some degree, the use of a third hand, I generally screw up this stool to a convenient height, and put my left (or right) foot on it, and, the patient's head being covered with a towel, I rest it against my knee to steady it; the wrist of the hand with which I am operating being also engaged in the operation on the other side, to aid in preventing any little unexpected movements of the head of the patient. I do not know that I could make any greater use of my own abilities in order to acquire a complete independence. Two fingers of the left (or right) hand are all-sufficient to hold open, and keep apart, the lids, and at the same time to steady the eye in any operation in which no considerable opening is to be made in it, as in cataract-solution operations, when one needle only, in the right (or left) hand is to be used. But, if an incision is to be made in the coats of the eye, the pressure required to be made upon them by the two fingers, which are placed actually on the eye, on the ocular conjunctiva and the margin of each of the two separated lids, would be too much for safety, and then a spring speculum and fixing forceps must be submitted to. If the middle and ring fingers are thus used, their soft ends are put between the lids and upon their margins and then separated, and with them the globe of the eye is pressed sufficiently backwards to secure its

not moving, the forefinger and thumb of that hand being still available. When an eye is fixed thus with two fingers, it is much more securely fixed than by means of any ordinary fixing forceps, which holds the eye at one point only of the mucous membrane, and, as this coat is loosely attached and easily torn, the instrument will only ineffectually prevent some of the movements effected by the rectus muscles, much less will it restrain the rotatory movements of the obliqui. Impressed with this defect of our fixing forceps, wishing to have the eye to be operated on more completely under my command, and considering that only *two* fixing forceps could effect my purpose, I, some years ago (*Ophthalmic Hospital Reports*, iv., 100), had forceps made which control *all* the movements of the eye completely; each blade is bifurcated with a U-shaped extremity, the width apart of the two ends of either blade being equal to one and a half or two diameters of an ordinary cornea, so that with it the eye is secured at *two* opposite and far apart points, by holding with it the conjunctiva and subconjunctival tissues at the customary most convenient distance from the margin of the cornea. These double ends (for the two blades of any forceps only supplement one another) are equivalent to *two* similar instruments. But the forceps must be carefully regulated in use, that the two ends be neither dragged upon nor depressed unequally; but, as we have no more than two hands, it is useful that the forceps may only employ one of them. The double hook used in general surgery in excising tumours is preferred to two hooks in separate handles for the same reason and for other reasons, one of them being that there is less chance of the double hook tearing out, as the force of the traction is halved, or unequally divided (as it is in the case of my double-ended forceps). When two separate and duplicate instruments can be used together, one in each hand, it is often very advantageous; it is no longer a question of economy of hand power, but traction is halved, or any other specific action of the two instruments is divided between them, and the action of either of them can be, and should be, always made supplementary to, and regulatory of, the action of the other, its opponent or coadjutor. With one hand one instrument “plays into the hand” of the other. When the best of all instruments only are used, “two are better than one.” In the final stage of enucleation *two* fingers are better than forceps, with which to move, or fix, and guide, and retain the eye-ball—better even than more than two fingers, which are unnecessary, and probably in the way. *Two* hands are similarly useful in removing very large tumours.

Two opposite iridectomies are often advantageous when one is insufficient; so that, in these later times, two are sometimes made at once, opposite each other, in the same eye, at the same



time of operating; sometimes even the corneal openings are made simultaneously with *two* broad needles, one in each hand, to avoid the escape of the aqueous humour by the first-made opening, when they are made separately, with an interval, however brief it may be, of delay. I am not now concerned in the consideration of the advantages of double iridectomies or other double or single (*e. g.*, squint) operations, but only in the advantages of using two similar instruments (in one or two handles) in some cases in which the objects of the operation, and how it is to be done, have been long ago determined.

There is a little-used, old-fashioned instrument in cataract instrument cases which has held its place in all the vicissitudes to which extraction operations have been subject of late years; for it is supposed to be sometimes indispensable, although as it is generally employed (too late to be of use) it might as well be absent. I mean the "sharp hook." *It is really a useful instrument; but we ought always to have not one but two of them for use together, and begin to employ them soon enough in the (unpromising) operation cases in which the sharp hook or some other traction instrument has been hitherto made use of as a last resource.* When I say *soon enough*, I am embarrassed by the conviction that I cannot quite define when the need for the instrument (instruments) has arisen. In mere operating practice is very much, and we learn to appreciate symptoms without bringing our minds even to the effort to describe them. Still, I think I can make this clear: when the need for a sharp hook (or of two sharp hooks) has arisen, the two sharp hooks are infinitely better, not only than one, but also than any spoon or scoop, or any other of the traction instruments, which are still used in these cases of emergency by all the best operators, although the practice of "out-scooping" cataracts has been long abjured by them.

The common sharp hook is as fine as may be to have sufficient strength and stiffness, though not much of either is required: it tapers, very gradually, from its proximal end in the handle to the very sharp point of the end hook. This hook is of about one-twentieth of an inch, and somewhat C-shaped; but the hook does not form more than one-third of a circle altogether. It was never much used, but its occasional use has never been abandoned. It was called for when the lens, in extraction of cataract, receded into the vitreous chamber instead of coming forwards in the regular way. In flap-extraction this recession, when it occurred, was as rapid—often instantaneous—as was the advance of the lens in a successful course. If it descended it was lost to view; and yet the operator, armed with the sharp hook, would seek for it, and then he abandoned the attempt, and there the lens was left and the eye closed. When a suicide

has thrown himself into the water we do not "drag" the river with but one hook. The operator may sometimes have succeeded in hooking the lens, more probably the retina, but of course he would not so be able to hold it. As I do not now operate by the flap-extraction, I need not determine exactly what I should do in it; but if I could not see the lens I think I would not seek for it, and if I could see it I would not attempt to hold and extract it with one hook only.

To proceed to von Graefe's extraction and my own experience. In the various stages of this operation, and under anæsthetic influence, there is no such need for hurry nor any such cause for sudden alarm: all is much more leisurely and safe. The lens advances slowly and gradually, gets a little more than half outside the corneal opening (and then it cannot recede); or it does *not* advance but a very little way, or not so far at least as to be half outside the corneal opening, and then, soon, pressure being still kept up, it will recede, and the vitreous advance. If the lens does not advance (and the corneal opening does not require enlargement) or begins to recede, or advances a little and then begins to recede, I take two of the sharp hooks, one in each hand, and, inserting one and then the other in the corneal opening, with the convexity of the hook downwards, pass them, held in the same way, both some distance directly back into the vitreous chamber and behind the lens till both are considerably past the middle of its back surface; then a slight rotation in the right direction (the necessary amount—a quarter—to be observed and calculated before the hooks are introduced, for their ends cannot now be seen) of the two instruments, one in either hand, brings them towards the lens. Their hooked extremities, being brought somewhat forward and then in a direction as if to withdraw them from the eye, are securely caught in the lens at its lower third or below the centre. From the moment when the lens is first caught in this way by one or other of the hooks, as at the same time a movement to withdraw them, and the lens of course with them, is made, the lens will rotate a little and incline to the right or left, and then we begin to appreciate the advantage of having two hooks, each one to aid the other: the lens, meeting with some slight resistance to its being withdrawn and removed from the eye, would inevitably fall off on one side or the other of the single hook, and the result would be worse than *nil*, for not only would the lens be lost, but it would be more or less comminuted, and so become more difficult of removal. Of course the two hooks may get a good anchorage in the body of the lens at the same instant, and this regulatory movement with the second hook may not be required. We have, in the next place, only slowly and very observantly, to withdraw the two hooks equally together, and the lens on



them; withdrawing a little more, comparatively, the one or the other of the hooks accordingly as one observes the lens to make a slight beginning of a rotation towards that hook, so as to stop that rotation and make the lens advance straightforwardly to its complete extraction. The patient, meanwhile, is kept profoundly under the influence of the anæsthetic, and if the eye is inclined to roll upwards, it is held down by fixing-forceps committed to the hands of an assistant.

Thus much concerning what I have called the “regulatory” action of the second (either) sharp hook. As to the “supplementary” action of the same I would explain it by a reference to a case in which I failed for want of it—*i.e.*, a second hook. It was some years ago, but I remember it very well because of the extraordinary end of the case. It was at University College Hospital, and I had not a second sharp hook, because one is now generally, and was then universally, supposed to be all-sufficient. At this hospital we have all necessary instruments, all that can be required—a complete set; but at Moorfields, where I first used a *second* sharp hook, I could have it without ordering it, because, with so large an ophthalmic staff, and so many instruments constantly under repair, there are duplicates of all kinds. In the case of University College Hospital, all had gone well at the beginning of the operation; the cataract was mature, there were no unfavourable symptoms, the globe was not soft, it was an ordinary senile cataract, commencing to form also in the other eye, and there were no posterior synechiæ, &c. But the lens, I do not know why, would not come forward but a very little, and soon began to go back, until the lower half of it was hidden behind the lower part of the iris. Some vitreous humour escaped, and the lens seemed inclined to descend lower and out of sight. I took the sharp hook, put it behind the lens in the vitreous chamber, and secured it and brought it forwards and upwards at last; then I essayed to bring it out, but it rotated and swayed from side to side, comminuting the back surface of the lens and detaching fragments which must be left behind. In order to make sure of not losing the lens altogether I had to keep the single sharp hook very firmly pressed into the lens, and, just as I thought I could extract it as a whole, it broke into two, and I brought out one half which was in front of the hook. (This would not have been so, I believe, if the pressure and traction had not been necessarily so great, and all at *one* place.) The other sank low down in the pupil, and another gush of the vitreous humour occurred. I was now afraid to attempt the removal of the second half of the lens, for it had become so much involved in the vitreous humour, and I did not think I could hold it with the (one only) instrument of whose powers, in my hands, I

had just been making trial. So I was glad to close and bandage the eyelids. The extraordinary end of the case is, that the man gained at last, and has at the present time, a perfect eye as regards vision. The half of the cataract, and the fragments also remaining in the eye, involved in the vitreous humour, rose towards the corneal opening and left a considerable space below them, and above the iris, of free pupil. In this situation they were found when I examined the eye next after the operation (the corneal opening was then already united and healed) and so they have remained ever since, only somewhat shrunk in the course of time. I think I could have removed the lens whole if I had had two sharp hooks to divide the pressure and dragging force between them, to prevent the sideways rotation and swaying to and fro of the lens, and the comminution and final split into two. All this a second sharp hook would have done for me, had I known it then and provided myself with a second hook. So would a spoon or scoop, some will say; but I will not omit to speak of the *comparative* advantages of the two hooks.

The two fine hooks occupy no appreciable amount of space in the corneal opening; they do not comminute the lens as one hook does, they transfix and hold the lens (it does not merely lie on them as a lens lies in the spoon or scoop which is used by most operators on these occasions), so that it can be brought out straightforwardly without need (or fear) of compressing it against the back of the cornea *and some of the iris, perhaps between them*. It is as different in practice as it is to land a fish with a gaff rather than to secure him only in a landing-net; the sense of safety and power is so very unlike in the two cases; it is a triumph even *before* the complete extraction of the lens which had threatened to escape, and a feeling very unlike that afforded by the partial security of a single hook or the danger of a scoop retaining the lens. Some soft cortical lens matter is almost certain to be left to be removed after the main body, nucleus, &c., of the lens is extracted; but this is commonly the case in all Graefe's extraction-cases, and the main body being out of the way, this soft lens matter may be extruded with a curette, as in other cases of extraction (or, at least, it should be attempted) in which there has been no mishap, or any fear of one. The rotatory tendency in a lateral direction, of which I have said so much in order to its prevention by using the two sharp hooks instead of one only, and by drawing one or the other of them a little more or less during the extraction—this very rotation which I have tried to avoid by all means, may be utilised in the final stage of the extraction when the lens is almost loose from the eye, by watching the opportunity of any inclination that it may have to rotation, and then turning it out so, sideways.



The advantages I have found in the use of the two sharp hooks I have described, in the cases in which, with a sufficiently large corneal opening, the lens will not advance, or having advanced a little way will not proceed through the opening, but seems rather inclined or has begun to recede, and the vitreous humour instead of it inclined to advance, are,—that an extraordinary amount of power is obtained, so that the lens cannot but be extracted; that it may be so well guided by a little more traction on one or other of the hooks; that by them it can be drawn straight out through the external opening without any harmful dependence and pressure upon parts opposite the traction instruments—the cornea and iris, against which the lens is often thrust violently in extracting by means of a spoon or scoop.

In a case recently at Moorfields, in which I operated for cataract in my usual way, von Graefe's method with my modifications—for every one now adopting this method of extraction has some particular fancies in which he allows himself to depart from the exact rules of the master,—all had gone well, and the corneal section was of fair size, but the lens after a little advance stopped, and beside it at one angle of the wound a prominent bead-like protuberance of the vitreous humour showed itself. I of course remitted all pressure with the curette; the vitreous protuberance remained. With the two hooks I drew the lens straight out through the opening in the way I have described; the vitreous humour retired altogether, and none of it was lost. This is almost the only case I know in which by any method when some vitreous humour has protruded so decidedly in an extraction-operation, the lens has been then afterwards extracted without loss of any portion of the vitreous humour. But if, as in most of my cases in which in alarm I have used the two hooks, some small portion of the vitreous humour is lost, and the lens has been extracted before or afterwards, it is very much better and more certainly done, I believe with the two very fine little sharp hooks than with a single larger instrument, and probably much more disturbance and compression of the neighbouring parts, besides the other disadvantages of a larger instrument and of one only.—*Lancet*, July 13, 1872, p. 38.

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### 63.—A FEW PRACTICAL REMARKS ON THE TREATMENT OF CATARACT.

By GEORGE CRITCHETT, Esq., Senior Surgeon to the Royal London Ophthalmic Hospital, Moorfields.

Before we proceed to the consideration of the advantages of the curvilinear, as contracted with the flap extraction, it may

be interesting and useful to describe the method of performing the modern operation or the Graefe extraction, as it is sometimes called. As it is a painful and tedious process, far more so than by the flap wound, it very much facilitates its performance to place the patient under the influence of chloroform. The lids must be fixed open by means of a wire speculum. I prefer the one modified by Professor Noyes, of New York; it has the advantages—1st. That it can be regulated with exactness by means of a screw which is on the nasal side, and is thus out of the way of the operator; 2nd. It does not press upon the globe.

The eye being sufficiently exposed it is fixed by means of a pair of forceps with broad serrated extremities that will take a firm hold of the conjunctiva, and of the sub-conjunctival tissue. Von Graefe's needle knife is made to enter the anterior chamber, puncturing the corneo sclerotic tissue at a point corresponding to the first dot of the dotted lines in Figs. 1 and 2, it is then passed across the upper part of the anterior chamber, and made to emerge at a point corresponding to the last dot of the curvilinear dotted line in Figs. 1 and 2. If it is wished to perform the true Graefeian operation, and to make the entire incision in the opaque fibrous tissue, the line indicated in Fig. 1 must be followed; if the cornea is to be involved in the

Fig. 1.

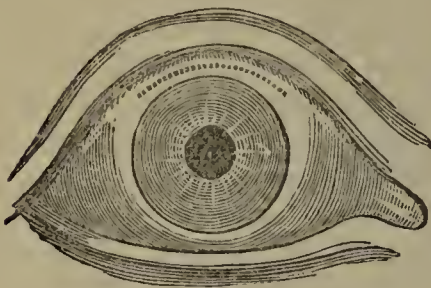
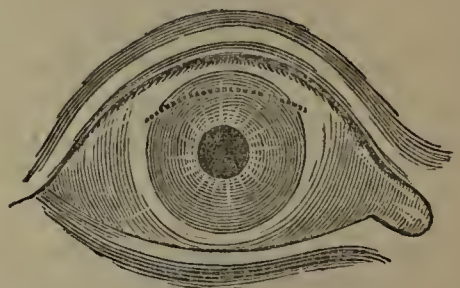


Fig. 2.



wound then the line indicated in Fig 2 is to be followed; when the section is made in the white fibrous corneo-sclerotic tissue a loose fold of conjunctiva has generally to be divided. After the curvilinear section is completed, the next step of the operation is to remove a portion of the iris; this may be done to a moderate extent so as to leave a loop-shaped pupil, or it may be cut away as far as the extreme angles of the wound, thereby leaving a much larger and broader pupil—the former is the plan adopted by the Berlin professor and followed by myself, and gives the best optical results; the latter may, perhaps, give greater safety to the operation and avoids the possibility of entanglement of iris in the edges of the wound. The next step in the operation is to open the capsule as freely as possible; this is a matter of some difficulty because the cataract in its



capsule is at this period in contact with the inner surface of the cornea, so that in passing the curette into the anterior chamber it is liable to penetrate the capsule at the edge, and close to the wound, and thus leave the anterior surface of the capsule entire and unlacerated. This, as we shall find hereafter, has an important bearing upon the question of secondary or capsular cataract and explains in a measure the greater frequency of this sequel after curvilinear extraction, than after the more ancient method by the corneal flap.

The next and third stage of the operation is the expulsion of the cataract. This may be accomplished very much in the same way as in the old flap operation. Usually it will pass through the curvilinear section very readily, but if it offer some slight resistance, it may be in consequence of the edge of the cataract impinging against the edge of the section instead of between the lips of the wound; if this occur the spoon of the curette must be gently pressed upon the posterior or upper lip of the section so as to press it behind the edge of the cataract when it is endeavouring to escape, and when it is in some measure entrapped by the sharp border of sclerotic wound, the edge of the cataract being thus brought between the edges of the curvilinear section it will usually emerge very readily under the influence of gentle pressure applied to the lower border of the cornea. If the cataract escape complete and entire the operation is finished; if, however, fragments of the peripheral portion remain behind, it is desirable to remove them if possible by means of gentle pressure; it is more especially important to remove any fragments that lie in or near the wound, as by getting between the edges they may seriously impede union. In spite of every effort it not unfrequently happens that some soft matter remains behind, and when the aqueous humour reforms, an opaque layer will be visible in the area of the pupil; if it be imprisoned between the layers of the capsules, it will remain as a secondary cataract, and will require a subsequent needle operation; if not, it will undergo absorption under the solvent action of the aqueous humour. If the fragments do not escape spontaneously it is better not to endeavour to remove them by the introduction of a spoon into the chamber; you thereby bruise and irritate the edges of the wound, you risk a rupture of the hyaloid membrane, and an escape of vitreous humour; and you often fail in getting away the fragments. It is important before closing the lids finally, to see that the edges of the wound are in accurate apposition, that there is no soft cataractous matter, and no blood clot interposing; a soft compress either moist or dry must then be fixed over the lids with a bandage. It will now be interesting to compare the advantages and disadvantages of the curvilinear

with the flap extraction. In stating the case for curvilinear extraction as compared with the flap operation I may mention in limine the safety with which chloroform may be employed; this has the double advantage of robbing the operation of all its terrors for the patient, and of enabling the operator to execute the various stages deliberately, and with precision, and without any spasmodic action of the muscles either of the eye or eyelid; in many cases this is a valuable adjunct and much increases the prospects of success. In the second place, the requisite skill for the performance of this operation can be more readily acquired and the execution of the various stages be accomplished with greater certainty and exactitude than in the flap operation. In the third place, the wound has an inherent tendency to keep in exact apposition, and in this respect contrasts most favourably with the flap, since the muscular contractions and spasmodic movement, which must necessarily separate this latter serve to keep the curvilinear section more completely in contact and closed. In the fourth place, and as a further result of the form of wound, the vitality of the cornea is not endangered, and union by the first intention rendered more certain. In the fifth place, prolapse of the iris which was so frequent and so serious a complication of the old operation is rendered impossible. In the sixth place, recovery is more rapid and more sure, and the patient may be safely permitted such an amount of liberty as removes all tedium and penance during the after treatment; a matter of some importance during the restlessness that often accompanies advanced age and an ill-disciplined will. We have thus, as it seems to me, stated six cogent reasons why the curvilinear section is preferable to the flap operation. We will now consider in what respects it is inferior to that operation.

In the first place it leaves the eye in a less perfect condition both optically and cosmetically than a perfect flap extraction with its small round central pupil. In the Graefian operation the pupil is large, loop-shaped, and excentric, and the curve of the cornea somewhat changed, so as to produce some irregular astigmatism. Although practically this does not entail any important inconvenience to the patient, yet it is a more mutilated and a less perfect organ in the one case than the other, and is more open to censure and criticism on the part of those who may be disposed to find fault. In the second place, it is more likely to be followed by a secondary or capsular opacity requiring the performance of a second or needle operation, respecting which I may have more to say hereafter. In the third and last place there is a liability in a certain small average of cases to the occurrence of secondary iritis of a very tedious, intractable nature, which produces serious impairment, and



even extinction of sight. This may come on in a week, a fortnight, and even three weeks after the operation; it may close the pupil, and bulge towards the cornea, or it may extend to the choroid, and produce softening of the globe, atrophy, and blindness. If this condition once sets in the prognosis is unfavourable. The cause of this secondary traumatic iritis is most probably the bruising and stretching of the angles of the wound, and the injury done to the ciliary nerves during the exit of the cataract. This would seem to be proved by the fact that an ordinary iridectomy is never attended with such unfortunate results, and that the flap extraction enjoys an equal immunity from such sequelæ. Moreover, I have met with more than one case in which the other eye seriously suffered from sympathetic ophthalmia, though I hope and believe this is a very rare complication.

The liability to traumatic iritis after this form of contraction is the most serious indictment that can be brought against the operation and one that has been observed by all who have practised it extensively; it is, however, of comparatively rare occurrence, and will probably become still more rare, as we learn to regulate the incision so as to permit the escape of the cataract without any force or stretching of the wound. In spite, therefore, of these three drawbacks—viz., the mutilation of the eye, the liability to secondary cataract, and the occurrence of iritis, it offers such numerous and important advantages over the old flap extraction, that it has established itself amongst a large majority of ophthalmic surgeons, and is destined to occupy the first rank as a method of removing cataract. This was amongst the last scientific bequests from our revered *confrère* Von Graefe, and it must go far to establish its position to know that, with his immense experience and judgment, he considered the operation as established and to be preferred before all others. Whatever the verdict of posterity may be, after prolonged experience as to its universal applicability, it must, I think, be retained as a most valuable contribution to ophthalmic surgery, as peculiarly applicable to those cases in which flap extraction is clearly contra-indicated, and as relieving both the operator and the patient from much needless anxiety. Highly, however, as I value this method of operating, and frequently as I have recourse to it, yet I cannot be unmindful of its weak points, and of the one serious source of failure; viz., the occurrence of traumatic iritis; and in any case in which this occurred in one eye, it admits of serious question whether some other method should be adopted when the second eye is operated upon. Following up the same train of thought as that which ultimately led to the curvilinear extraction, we may ask ourselves whether there is any plan whereby the

objections to the flap extraction on the one hand and to the peripheral curvilinear extraction on the other, can be escaped—the main objections to the former being, as you remember, a liability to prolapse of iris, and non-union of the flap; the main objection to the latter being a tendency to iritis. When in Brussels last autumn I saw an operation performed at the clinique of my excellent friend and *confrère*, Mons. Warlomont, which seems in a great measure to meet this desideratum. It consists in moving the curvilinear section lower down, so as to be entirely corneal.

In performing this operation Graefe's needle knife is employed; it is made to traverse the anterior chamber just above its centre, and to cut its way out by turning the edge gradually forward, so that the curve of the section being of the same shape as in the operation of Von Graefe, shall form a line rather above the edge of the pupil. After the capsule is opened the cataract, however hard and fully formed, will readily escape, leaving a small round normal pupil. Since my return from the Continent I have performed this operation several times both at the hospital and in private practice, and I find it possesses many advantages. In cases where it is undesirable to administer chloroform it is a very painless operation; it is comparatively easy of performance, it inflicts the least possible injury on the eye; the curvilinear form of the wound renders prolapse almost impossible, and favours union of the cornea, the pupil retains its circular form and central position, and its optical conditions are good; that form of traumatic iritis that sometimes occurs in the corneo-sclerotic section is altogether avoided, so that the more serious dangers both of the flap extraction and of Graefe's method are escaped. The chief risks are the liability of the iris to fall forward and attach itself to the curvilinear and central section, and the tendency to suppuration which all corneal wounds exhibit in certain conditions of the constitution. The eye is more irritable and painful, and makes a slower recovery than in the more favourable cases of corneo-sclerotic section, in which the restoration is often remarkably quick. An extended experience is needed before we pronounce a final verdict upon the merits of this curvilinear corneal section, but it certainly possesses some important advantages, and may be adopted just in those cases in which the operation of Graefe may be contra-indicated, as, for instance, where chloroform is inadmissible, or strongly objected to, or where an eye has failed through traumatic iritis, or choroido-iritis. When the subject comes to be calmly considered in all its details, and by the light of an extended experience, it will probably be thought to be unscientific and inexpedient to adopt any one method to the exclusion of all others. The controversy



must in all probability chiefly turn upon the question whether the cornea or the white fibrous tissue connecting cornea and sclerotic is the most prone to heal, and can be most eligibly selected for the section. Each of these tissues is liable to dangers from which the other is exempt, so that if we can learn to select the particular form of operation suitable to a certain state of constitution, we shall increase the probabilities of success, or where one method has failed, if we choose the other for the companion eye, we in this way give our patient a better chance, or if we operate on both eyes at the same time we shall use a wise discretion in selecting one method for one eye and the other for its companion. I believe the curvilinear form of section ought in future to supersede the flap, as being more scientific and more prone to heal, but if we would reach the highest average of success we must vary the situation of the curvilinear section in accordance with the peculiarities in the reparative and healing powers of our patient. For those who are prone to suppurative inflammation the fibrous tissue should be selected for making the section; for those who show a tendency to plastic exudations the corneal wound is indicated. I think this recent method may be denominated "extraction by median corneo-curvilinear section." It was originally suggested and performed by Lebrun, of Brabant.—*Medical Press and Circular*, April 3, 1872, p. 291.

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#### 64.—TWO CASES OF AMAUROSIS TREATED BY THE SUBCUTANEOUS INJECTION OF STRYCHNIA.

Under the care of CHARLES HIGGINS, Esq., at the Central London Ophthalmic Hospital.

*Case 1.*—Joseph W., a single man, a watchmaker, has been in the navy, and seen a great deal of foreign service, especially in India. Twenty years ago he had a sore on his penis, but no symptoms of syphilis. Has left the navy ten years. His health has generally been good. He has been in the habit of smoking a large quantity of tobacco, and has drunk a great deal of spirits. For two months he has noticed his vision becoming impaired in the same degree in both eyes, accompanied by a feeling of fulness in the eyeballs, but no pain.

February 10th. Can make out letters of No. 8 of Snellen's types at about five inches with either eye. On examination with the ophthalmoscope, in the left eye the outer half of the optic disc is pink and white in patches, the inner half uniformly pink; the retinal bloodvessels, especially the arteries are thin, and few in number. The right eye is the same as the left, with the exception that the retinal vessels appear larger and more

numerous. In both eyes the vessels appear displaced towards the inner side of the optic disc. Ordered pulv. opii gr.  $\frac{1}{2}$  in pilula ter die.

24th. No improvement. One-fortieth of a grain of strychnia injected beneath the skin of the temple.

28th. One-thirty-second of a grain of strychnia injected.

March 3rd. One-thirty-second of a grain of strychnia injected. Reads Snellen  $6\frac{1}{2}$  at six inches with both eyes.

30th. Strychnia has been injected twice a week, the quantity having been gradually increased to one-eleventh of a grain. Reads Snellen  $3\frac{1}{2}$  at about six inches.

April 13th. Injection has not been repeated for a fortnight. Reads Snellen  $3\frac{1}{2}$  at about eight inches with both eyes, and says he can do his work as well as ever he could. Examination with the ophthalmoscope shows little change. The outer half of each disc is of an uniform pinkish-white colour, not patchy as before; the inner halves are still uniformly pink. The retinal blood-vessels present no change.

*Case 2.*—Charles K., the father of three healthy children, was formerly in the army, is now a cabinet-maker. Thirty years ago he had a sore on the penis, accompanied by suppurating bubo, but no symptoms of syphilis. Six months ago he had a severe blow on the head from a piece of timber. About the same time he had a severe attack of rheumatic fever. Five months ago he noticed his sight failing; both eyes became affected together, and loss of vision has continued to progress in both equally. His health is usually good. He is a great smoker, and has drank a great deal of spirits.

February 24th. Right eye reads Snellen 12; left eye reads Snellen 15. With both at six inches he cannot distinguish colours. On examination with the ophthalmoscope both optic discs appear white and anæmic; the retinal vessels, especially the arteries, are thin and few in number, the change being more marked in the left eye. One-fortieth of a grain of strychnia injected beneath skin of temple.

27th. One-thirty-second of a grain injected.

March 2nd. Right eye reads Snellen  $2\frac{1}{2}$  at eight inches. Left eye reads Snellen  $6\frac{1}{2}$  with difficulty with a biconvex lens of fourteen-inch focus. Right eye reads Snellen  $1\frac{1}{2}$ ; left eye not improved by any lens. Can distinguish colours.

20th. The injection has been repeated twice a week, the quantity of strychnia having been increased up to one-eleventh of a grain. He still reads the same letters.

April 10th. Has not attended for three weeks. Vision remains the same as on February 27; the left eye has never improved as much as the right. Examination with the ophthal-



moscope shows little or no difference, excepting that I fancy the retinal vessels are rather larger.

*Remarks.*—I look upon both these cases as examples of tobacco amaurosis. Both certainly improved much under the treatment. Whether the good result was brought about by the use of the strychnia or by abstinence from smoking and drinking I am not prepared to say. I may add, however, that I so frightened both these patients at their first visit by telling them what probably would happen if they continued these habits, that I have reason to believe they ceased to smoke and drink to a very considerable extent. In the first case the strychnia seems to have done all the good, as the patient had been under treatment by opium for a fortnight before its injection.—*Medical Times and Gazette*, July 20, 1872, p. 68.

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#### 65.—PRACTICAL OBSERVATIONS IN OPHTHALMIC AND AURAL SURGERY.

By ERNEST HART, Esq., late Ophthalmic and Aural Surgeon to St. Mary's Hospital.

It will be useful to group together, for the sake of emphasis, a numerous class of cases in which I have seen mischief arise from slowness to employ atropia locally. Looking through a long list of cases, I find that I have employed atropia-drops in the course of the treatment of 1,523 out of 2,710 consecutive cases of disease and injury of the eye (a list not including, of course, affections of the ophthalmic appendages); and I pass in review a long array of cases in which I had to regret that it had not been used before the patients came into my hands. Two of the most recent consultations which I have entered, were cases of injury to the eye, respectively at Paddockhurst and at Brighton, during the last month. In both cases, the treatment had been unexceptionable, except that atropia had not been used; and, though the globes have been saved, the fixed and closed irides will make the possibilities of vision depend upon iridectomy under not very favourable circumstances. In the very last consultation on a case of iritis, of which I have notes, the treatment had equally been conducted without dilating the pupil. Mercury, leeches, and lotions were in use; a darkened room and an antiphlogistic diet were all in use when I was called; but the vision was extremely misty, the pupil contracted, and the patient in proportionate danger of "false pupil" from the effusion of lymph, and of loss of sight.

Now I do not think that I shall be using an exaggerated form of expression, or going beyond the strict and well-balanced weight of words which is necessary to give due force to the

fact to be conveyed, if I say that we could, in the treatment of ophthalmic disease, better afford at this day, so far as our knowledge of disease and means of mastering it extend, to dispense with all other drugs, lotions, and applications put together, than with this one *topical* medicament. Let us consider what atropine does for the inflamed eye. It allays local sensitiveness, and removes local spasm; it gives to the eye and to its internal muscular apparatus—iris and ciliary muscle—physiological rest, the greatest of all curative means. Nor does it do this only, but, in dilating the pupillary aperture, it drives far from us the bugbear which long haunted the ophthalmic surgeon, and which still pursues those who are not sedulously active in the use of atropia—closure of the contracted pupil by an adherent plug of lymph, and gluing of the uveal surface of the iris to the lens. It would rob the consulting-surgeon of a great many profitable but trying operations, if the atropine eye-drops were ready in every surgery, not only on all emergencies, but for the exigencies of daily practice. It is as safe a rule in ophthalmic practice to use an atropine drop when in doubt, as in whist to play a trump. I can hardly think of more than one absolute contraindication, and that is the existing oval dilatation of the iris in glaucoma. There are, of course, also mechanical contraindications, as in peripheral wounds of the cornea with hernia of the iris, where to dilate the iris is to increase the peripheral protrusion; but even here, the moment the corneal gap is healed, atropia becomes of the first necessity. But in all cases of iritis, in contusions and injuries of the eye, in corneitis, purulent ophthalmia, scrofulous ophthalmia, and deep-seated mixed inflammations of the eye, the local instillation of a solution of atropia is the most precious of therapeutic means. The most useful strength is, I think, expressed in the formula: Neutral sulphate of atropia, two grains; glycerine, five drops; distilled water, an ounce. The frequency of the use of the drop must vary with the facility and rapidity with which it acts. Where the iris has become much inflamed before local treatment is adopted, it is sometimes very indocile, slow to respond, and hard to dilate. Then the measure of frequency must be the amount of resistance, and perseverance must be the rule of treatment. In the treatment of keratitis and minor cases of deeper inflammations, one application a day, or at most two, will suffice; and presently, once in two or three days. It will be enough then to keep the pupil dilated, and the ciliary apparatus at rest and free from tormenting spasm. The present result of the most careful observation of the origin and cause of ophthalmic disease, pursued with the advantages of the improved methods of optical diagnosis now at our command, is to simplify our treatment, and to ostracise a majority



of superfluous agents of medication. With a little cotton-wool, alum, and glycerine, hot and cold water and atropine, and a pocket-case of instruments, we can treat with a previously unattainable success nearly the whole range of ophthalmic diseases. I am speaking of local and surgical treatment. Ointments, poultices, caustics, irritants, scarifications, venesections, blisterings, and setons, may be looked upon almost as things of the past. The medical, dietetic, and hygienic treatment are favourably modified, and simplified to an almost equal extent, by the intelligent study of diathetic indications; but the whole shopful of topical applications may be left aside by the surgeon who will thoughtfully apply the vast resources of the few simple agents I have named; and of them all, atropine is greatest. I will not undertake to say that mercury is useless in the treatment of some forms of (syphilitic) iritis; but I will affirm that I have repeatedly seen iritis occur and run a very severe course in patients previously and at the time already under the influence of mercury; and that, in a long series of cases which I treated by atropia and careful dieting only, and without mercury, during five years at St. Mary's Hospital, the results were so excellent that I could not affirm that they would have been improved by the most guarded and judicious use of mercury. A number of these cases were seen by my colleague Mr. Gascoyen, who took part in the experiment.

It is possible, though not easy, to abuse atropine. It must not be used, as I have said, in glaucoma or in peripheral wounds of the cornea. A case or two of troublesome constitutional symptoms, through absorption of the excess by the lacrymal mucous membrane, have been recorded. This is never likely to occur with ordinary care, and I have never seen it occur; but it may be well to bear it in mind in treating infants. The most convenient and unfailing application may be made by the use of the atropised gelatine discs which I introduced a few years since, and which are now largely used in this country and abroad. They are always ready, do not spoil by time, and are clean and precisely dosed, each disc containing as much as a drop of the solution I have mentioned.

I do not think I have spoken too strongly of the advantages in practice of a free use of the local instillation of atropine. In cases of doubt, it will often throw much light upon an obscure condition; and those not much given to study ophthalmic cases will be presently surprised to find how often it will, in a dubious case, first display, and then rectify after a few days, recent and unsuspected adhesions of the iris. In the average run of ophthalmic cases, whether for the treatment of a large proportion of the inflammatory disorders of the eye, by securing anæsthesia and physiological rest, or in the diagnosis of optical

defects, it is of ever-recurring use. Few things would tend to curtail the extent and to increase the success of consulting practice, more than a judicious prodigality by the general practitioner in the local use of atropine.—*British Medical Journal*, April 27, 1872, p. 445.

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#### 66.—ON SOME AFFECTIONS OF THE EAR.

By Dr. W. B. DALBY, Aural Surgeon to St. George's Hospital.

*Foreign bodies in the meatus* are generally put there by children at play, and are chiefly stones, beads, peas, and the like. With regard to their removal, two points should be borne in mind. In the first place, that as the meatus is lined by skin continuous with that of the auricle, provided the edges of the object are not sharp, they may remain there for an indefinite period and do no harm. In the second place, that very considerable injury is often done by injudicious efforts which are made to extract them. It is not, however, to be understood from this that no attempts should be made to remove them. But no instruments should be used, excepting when, with the mirror on his forehead, light is reflected down the meatus, and the operator, with both hands free, can see what he is doing. The first thing to be done in any case is to syringe the ear; and in doing this the nozzle of the syringe, which should be fine and probe-pointed, is to be kept at the upper part of the meatus, so that the returning stream of water may bring it away. This not happening, if it is near the external orifice, or at any rate not more than half way down the meatus, and especially if it be of a soft nature or of an uneven shape, it may often be laid hold of by a pair of ring polypus forceps. A loop of silver wire may sometimes be passed beyond it, and by pulling at this it may be moved outwards; or the noose of a Wilde's snare may be used in the same way, and the body sometimes by this means be secured and extracted. Young children must have chloroform, not because they are being hurt, but because they will not keep quiet; and without this it impossible to do any good. Adults will sit still in a chair, as they are not put to any pain. If the foreign body cannot be removed in one of these ways, or if it have passed more than half way down, for the reason shown when speaking of the shape of the meatus, any further attempts will assuredly only have the effect of sending it further in, and will frequently result in a rupture of the tympanic membrane. The ear should be occasionally syringed, and the foreign body will gradually approach nearer and nearer the orifice, until it finally drops out. It may be weeks or even months, before this happens, but, if it be left alone, it will do no harm. If the tympanic membrane be unfortunately rup-



tured, this is generally followed by suppuration in the cavity of the tympanum, a discharge from the ear lasting a long time, and more or less permanent deafness. After such an accident, if there be much swelling of the meatus and pain, a few leeches in front of the tragus, followed by fomentations, will be advisable. Anything more energetic than the treatment suggested in the case of foreign bodies in the meatus is thought more likely to prove harmful than beneficial, notwithstanding the numerous instruments that have been brought forward, it is said with the object of getting behind the body and removing it.

In addition to the *deafness induced by cerumen* when it blocks up the meatus, there is frequently *tinnitus*, and sometimes of the most distressing character. Anything which causes undue pressure of the stapes on the fenestra ovalis will produce it, and in this case it is brought about by a plug of wax directly in apposition with the tympanic membrane, thus transmitting pressure through the malleus and incus on to the stapes. The same exciting cause will often induce attacks of giddiness, and this symptom has occasionally existed for years without eliciting a suspicion of its cause, the patient in the mean time having been submitted to all sorts of treatment, without, of course, receiving any benefit. This arose from the fact that there was no accompanying deafness to point to a cause, for in these cases the plug of cerumen does not entirely cover the membrane, and so there is ample room for vibrations of sound to fall upon it and be transmitted as before. As a rule, however, there is deafness as well. Accumulations of hardened cerumen from continued pressure on the membrane, will occasionally produce ulceration of this structure, thus becoming the direct cause of perforation; and in the same way they have occasionally induced partial absorption of the walls of the bony meatus. There are some preparations showing this in the museum of the College of Surgeons. The only legitimate way of removing cerumen is by the use of the syringe, and the best form is one where the nozzle is very small and can be removed from the other part by bayonet action. This form of syringe is quickly filled, and so time is saved; and from the small size of the nozzle the meatus is not blocked up by it. In so simple an operation as syringing the ears there is a skilful and an unskilful method of doing it. The auricle should be drawn upwards between two fingers of the left hand, so as to put the whole meatus as far as possible in a straight line, and the nozzle of the syringe should be kept in close contact with the roof of the meatus. If the secretion is very much hardened, it is well it should be softened by a little warm water poured into the ears for a few nights successively. At intervals during the

syringing the ear should be examined with the speculum to see if the canal is clear; for if it is so, considerable irritation may be set up by the stream of water falling with force upon the membrane. It is not very rare to meet with patients who, having had their ears syringed vigorously when there was nothing to bring away, have had inflammation of the membrane set up, which has ended in a perforation. The wholesale way, therefore, in which people get their ears syringed as soon as they feel deaf is to be deprecated.

One of the most troublesome affections of the external auditory meatus is the formation of small *abscesses*, which may occur at any situation in this canal. They do not seem to be confined to persons of any particular age or constitution. The first symptom is pain in the ear, which in a few hours becomes so acute as to put sleep out of the question. After twenty-four hours or so the meatus in its entire extent will be swollen; sometimes the whole external ear will be enlarged, red, and tender to the touch. The movements of the jaw occasion great pain. There is more or less deafness, in proportion to swelling. If the patient be seen within a day or two of the commencement of the attack, the greatest benefit may be afforded by two or three leeches placed in front of the tragus, just over the articulation of the lower jaw, and followed by fomentations: in this way the affection may be cut short occasionally. When the leeches are applied, the meatus should be filled with cotton wool to prevent the blood getting in. If not checked in the early stage, the state of things described will increase in severity for several days, and, on examining with a probe, one point will be felt to be far more acutely tender than any other part of the meatus. The whole meatus is so swollen that it is not possible to see the position of the abscess with a speculum. This point having been distinctly localised, the sooner an incision is made into it the better, as by this the tension of the parts, to which the agonising pain is due, is relieved. The best knife to use is a curved sharp-pointed bistoury, and a free incision should be made as it is withdrawn. After this the patient gets well in a few days. Very frequently, however, he is no sooner well of one abscess than another commences to form, and all the suffering has to be endured again. It is this recurrence that makes the affection such a troublesome one. Sometimes six or seven abscesses will occur successively in the same patient, obliging him to remain in the house for as many weeks, and causing an amount of suffering altogether disproportionate to the gravity of the illness. In recognising this occasional peculiarity in these cases, caution will be observed in promising a patient that he or she will be well within a certain time.

The diffused form of *inflammation of the external auditory*



*meatus* differs from the circumscribed, inasmuch as it does not terminate in abscess, and, as its name implies, in being diffused throughout the integument of the canal. Since the outer cuticular layer of the tympanic membrane is continuous with that lining the meatus, there is always a certain amount of risk lest this membrane become implicated, and for this reason this affection is more serious than the one just considered. It is not strictly correct to describe it as being of two kinds—viz., acute and chronic,—because, although the attacks are acute at one time and chronic at another, their gradations so insensibly pass into one another that an artificial division is practically useless. Children are especially liable to it; and as it is generally neglected among the poorer classes, with them it is not unfrequently the origin of a perforation of the tympanic membrane. It often succeeds the exanthemata, although not nearly so commonly as purulent catarrh of the middle ear spreading along the mucous membrane from the throat. The diffused form of inflammation of the external meatus commences with a feeling of uneasiness and itching just within the orifice, which very soon becomes red, tender, and swollen; and there is pain during mastication. These symptoms increase in severity, until, with the appearance of a discharge, the pain ceases, and the swelling gradually subsides.

Such is briefly an account of its acute stage, which usually occupies a week or ten days. The treatment is palliative—viz., leeches and fomentations; but as this affection does not occur to persons in a good state of health, the diet should be attended to, and a change of air, if possible, be resorted to, for it is most desirable not to let this condition become a chronic one, as it is most apt to do.

*Eczema* is a common affection of the outer ear, and may be acute or chronic. After repeated attacks (for a patient who has once suffered from it will be very liable to it again) the auricle becomes very much thickened, and the entire meatus shares in the same condition. If there is any deafness, it is only so far as may be accounted for by the narrowing of the canal, which takes place when the disease has lasted for a long time. Like eczema in any other part, it is very obstinate; but if the patients will consent to keep to rules of diet, including an absence of stimulants, they will get well. This is more important than any external applications. Of these, very mild mercurial ointments are, perhaps, the most useful; and before they are applied the parts must be carefully dried.

*Exostoses* are not unfrequently met with in the external auditory meatus, and are not confined to any situation in its course. They are solid bony growths, covered with periosteum. Sometimes there are two or three at one time, and they scarcely ever

have a pedicle. On this account, and from their position, they are not easily removed by an operation. Fortunately, this is not often necessary, as they seldom entirely close the meatus, and so do not interfere very materially by their presence with the hearing, but do so indirectly by secretion of cerumen and epidermis collecting behind them; and in this way may be very troublesome. They have been considered to be the result of a gouty diathesis; and the long-continued use of iodine applied locally has been said to induce a diminution of their size. No evidence of either of these propositions can be found. These osseous tumours, or enlargements of the bony meatus, as the case may be, are of very slow growth, and acquire a considerable size without the patient discovering them.—*Lancet*, Aug. 24, 1872, p. 249.

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#### 67.—FOREIGN BODIES IN THE EAR.

Dr. LÖWENBERG has published, in the *Berl. Woch.* three very instructive cases, accompanied by judicious remarks, illustrating the proper modes of removing foreign bodies from the external ear. The important fact made evident by the cases is the prejudicial nature of the rough or ill-directed attempts at removal which are generally instantly made when the foreign body is complained of. In fact, such attempts may give rise to inflammatory effusion, which forms a bulging ring around the foreign substance. Dr. Löwenberg also shows that the latter may remain in the outer ear for several weeks without occasioning great discomfort. Also that the foreign body, or the instruments used to remove it, sometimes perforate the membrana tympani; but that this membrane may eventually recover its integrity. We also notice that such perforation may be proved by the reappearance of water injected into the ear at the mouth and nose, or by forcible expiration when both the latter are closed. In the latter instance bubbles of air will be noticed in the water left in the meatus.

The author relies chiefly on injections of water, and, these failing, on the *agglutinating* method. To carry out the latter he uses a stem delicately surrounded with lint, the end of which, a little unravelled, is dipped in carpenter's glue. None of the glue should touch the sides of the stem. The latter is left from fifteen minutes to an hour, and the fastening becomes so firm that the foreign body can be extracted by careful management. A bead thus removed has remained attached to the stem for three years. Two cases, after all other means had failed, proved successful with the glue; and a third at once yielded to injections.

A strange circumstance was noticed in a boy of five years.



A shirt button had been introduced into the left ear, and was eventually found there; but when the child came under Dr. Löwenberg's notice, the *right* ear was complained of. He saw nothing in it, and when, according to his custom, he explored the supposed sound ear, the button was seen lying against the membrane. The explanation was that, after the first attempts, the boy became so alarmed and confused that he presented to the first surgeon called the right ear, which, in consequence of manipulations, became inflamed, and was looked upon as containing the foreign body.

From the details of the cases it would appear that Dr. Löwenberg is in Paris quite a specialist as regards these extractions. He seems, however, to undervalue the delicate instruments which have been devised for the purpose. No doubt roughness or blind explorations are mischievous, but proper instruments, used with gentleness and with due regard to the anatomy of the part, should not be despised. The gluing method is very attractive through its simplicity and proved efficacy. The author especially directs attention to the position of the head when injections are used. It should be such as to favour the *vis a tergo* of the stream of water.—*Lancet*, June 1, 1872, p. 774.

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#### 68.—ON THE TREATMENT OF EXANTHEMAL CATARRH OF THE TYMPANUM.

By Dr. JAMES PATTERSON CASSELLS, Glasgow.

[These cases are of the greatest importance. They are liable to be overlooked to some extent in consequence of the apparent relative urgency of the accompanying exanthem.]

The acute form of this catarrh is usually met with in children during the course of the febrile attack, particularly when the naso-pharyngeal affection is severe; and is ushered in by increased restlessness and pain, which is referred to one or both ears, with more or less functional disturbance of the organ. After the lapse of a few hours, during which the suffering is intense, an escape of purulent fluid takes place from the external meatus, and relief is obtained. Usually the discharge lasts a few days, and then ceases; the hearing is restored; and the attack passes away without apparently doing damage to the delicate structure.

But this is a favourable termination, for, at other times, the restlessness and pain are quickly followed by symptoms indicative of cerebral disturbance, such as convulsions and coma, which, if the patient is already exhausted, not unfrequently terminate fatally. Many of the patients die apparently from

inflammation, or effusion within the head; they have violent headache, with furious delirium; which is followed by coma and death. What proportion of the cases, which have this unfavourable termination, have also an acute catarrh of the ear, with some of the pathological results presently to be referred to, it is impossible to determine, in the absence of data. But it is in the experience of most general practitioners, who have observed an epidemic of scarlatina, that young patients, who have suffered from violent headache with furious delirium, and sometimes coma, have had a sudden relief from their apparently hopeless condition, after a discharge of pus-like fluid from the external auditory meatus; so that it is just to assume that the morbid contents of the tympanum, in such cases, exercising undue pressure upon the fenestræ of the labyrinth, had given rise to the alarming symptoms; and that these had disappeared when, by rupture of the membrana tympani, that pressure was removed, and the contents of the tympanum permitted free egress.

But pathological anatomy shows, that this favourable termination does not always take place; that the membrana tympani resists the pressure from within; and the pent up morbid matters, unable to make their escape by the Eustachian tube, already most effectually closed by a concentric thickening of its lining membrane, find their way to the base of the brain, either by breaking through the fenestra cochleæ, or some imperfectly ossified part of the walls of the tympanum; thus causing the death of the patient.

Cases occur, however, in which not only does the membrana tympani resist the pressure, but no part of the tympanic walls gives way; and the more urgent symptoms already referred to subsiding, the patient recovers with the hearing power more or less impaired. The disease now becomes chronic; the morbid contents of the tympanum becoming more or less inspissated or organised, and the lining membrane and osseous walls hypertrophied, (in addition to ankylosis of the ossicles, or of the base of the stapes to the fenestra ovalis) great, and in many cases, irreparable damage is done to the organ.

It is not considered necessary to multiply proof in support of what has been advanced, it is sufficient to call to mind the anatomical structure and relations of the petrous bone—indeed, of the whole temporal bone. The walls of the tympanum are always thin; frequently imperfectly developed; and have free inter-communication, both vascular and nervous, with the important organs and structures which surround it; while every pathological change in the delicate membrane lining this cavity, from its intimate connection with the periosteum of its walls, gives rise to a disturbance of their nutrition and its consequent



evils; every inflammation of the lining membrane of the tympanum and of the mastoid process is an inflammation of the periosteum; every catarrh of the same a periostitis; whilst, with acute processes, the lining membrane is more prone to ulceration, and the periostitis leads more frequently to atrophy; to inflammatory softening; and to caries of the bone; thickening of the mucous membrane, and hypertrophy of the osseous structures readily develop themselves, when the inflammation has become chronic. Moreover, the pathological collections of this country and the continent, as well as the writings of English and continental aurists, show with what alarming frequency death results from a simple exanthemal catarrh, which has run its course untreated.

It is obviously impossible within the limits assigned to this paper, to indicate all the varied morbid appearances which the membrana tympani presents in this form of aural catarrh; but usually if the membrane is inspected in the acute form, a proceeding which should never be neglected in the course of scarlatina, especially where symptoms appear indicative of an extension of the throat affection along the Eustachian canal, it will be found of a yellow or brownish yellow hue, particularly at the lower segment; lustreless and opaque; with more or less convex bulging; and with one or more injected vessels running parallel to the handle of the malleus; while a zone of minute vessels occupies the circumference of the membrane, or may even cover its whole surface, giving it a red and velvety appearance. If the membrane, however, is about to give way before the pressure of the contents of the tympanum, the part at which it is likely to rupture, will present a bright or greenish yellow colour, and is much projected externally.

In the chronic stage the appearances of the membrana tympani are totally different. The colour may be white or stone-grey, with a yellowish tinge; lustre more or less gone; and opaque in whole or part; sometimes the transparent portions permitting the observer to verify his diagnosis by an inspection of the contents of the cavity, or of its lining membrane. The curvature of the membrane may be normal or present any degree of departure from that standard, up to well marked convexity, in whole or part; and, in addition, some injected vessels ramifying upon its surface; although these latter are not of frequent occurrence in the chronic stage of this disease.

In the foregoing brief and necessarily imperfect account of the disease, the appropriate treatment in the acute stage, naturally suggests itself; viz., early and free incision of the membrana tympani; and in the chronic form, as part of the treatment having for its object the removal of the unorganised deposit contained in the tympanum, incision of the membrane as clearly indicated.

The operation is performed, with a delicately constructed lancet-shaped knife, in the following manner:—

Having obtained a good view of the *membrani tympani*, the knife is passed quickly through the membrane, near to the inferior margin of the posterior inferior quadrant, cutting upwards for about 5 m.m. (3 lines) in the posterior segment; in acute cases incising by preference any part of the membrane more protuberant or convex externally. In the first introduction of the knife, care must be taken not to wound the inner wall of the cavity, else the hemorrhage which follows, and the coagulæ that form, may mar the success of the operation. In order to avoid this, it is advisable to Politzerise, immediately previous to the operation, or to cause the patient to inflate the ear by the Valsalvian method at the moment the knife is introduced, when either proceeding is admissible. Usually some of the morbid matter escapes immediately on the withdrawal of the knife; the quantity depending upon the consistence of the contents of the middle ear.

The after treatment in acute cases, consists in occasional Politzerising to facilitate the outflow of the purulent fluid, and the observance of scrupulous cleanliness; after a few days, if the discharge does not cease, the instillation of a warm astringent lotion (*zinci sulphas gr. ii. ad. ℥i. aquæ*). The necessity for a repetition of the operation, within a few days, may arise; this is to be determined by circumstances, and needs no further consideration.

When the case is already chronic, before coming under observation, and the signs of occlusion of the tympanum by morbid deposits, are well marked; or the history is suggestive of such a condition; the following method of treatment is adopted. It is a combination of that of Politzer for chronic mucous catarrh, and of Hinton for chronic mucous accumulation around the auditory ossicles.

Having ascertained that the Eustachian tube is patent, or made it so, either by Politzer's method, or by passing a stream of compressed air through it, a small quantity of a perfectly transparent and warm (100° F.) alkaline solution (5 per cent. soda of bi-carb.) is injected daily, through a vulcanite catheter into the tympanum, in order to soften the morbid matters retained there. After each injection Politzerising, with the object of lessening the congestion and hypertrophy of the membrane, and of the Eustachian tube; and in some degree diminishing the probability of secondary inflammatory action following the operation of incision, as well as rendering the Eustachian canal more patent. After some days of this preparatory treatment, the operation is performed in the manner already described. Immediately after the incision the tym-



panum is washed out with a warm solution of common salt, either with a syringe, the point of which is tipped with india-rubber, and fits tightly into the anterior orifice of the nasal passage; or through a vulcanite catheter introduced into the Eustachian tube of the affected ear. The latter method *must* be used unless both ears have been operated upon at the same sitting, when the former is preferred. Usually, by this proceeding, some of the morbid deposit is forced out into the external meatus, from which it is easily removed, either by the forceps or by gentle syringing. The tympanum is then washed from the meatus ext. by a large syringe, the nozzle of which fits accurately into the orifice of that canal, and is prevented from injuring it by a protecting ring of rubber affixed to its point. The patient is directed to lean forward, and to breathe through the mouth; the syringe filled with the warm solution is applied to, and pressed into the orifice of the meatus, and the fluid forced through the ear. At first the pressure must be gentle, until the fluid begins to run from the anterior orifice of the nasal passage, then more pressure may be employed. Oftentimes considerable force is necessary to dislodge the deposit; the quantity removed in many cases is very large. This treatment is repeated every day till the incision closes.

Sometimes patients complain of giddiness, which soon passes off; this symptom has been ascribed to the coldness of the fluid injected, but it appears rather to be caused by pressure upon the fenestræ of the labyrinthine wall.

With the fluid warm; the incision large and free; the pressure prudently applied; and the Eustachian canal *open*, no danger need be apprehended; further, the operation and the after treatment may be repeated, without in the slightest degree injuring the organ.—*Glasgow Medical Journal*, Aug., 1872, p. 470.

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#### AFFECTIONS OF THE SKIN.

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### 69.—ON THE GENERAL PRINCIPLES OF CUTANEOUS THERAPEUTICS.

By Dr. TILBURY FOX, Physician to the Skin Department of University College Hospital.

[There are many purely local diseases which require local treatment only, but, on the other hand, these diseases may occur in a badly-nourished subject, and consequently demand a judicious combination of general and local treatment.]

There are one or two special points in the therapeutics of these cases. I may say generally that in these diseases the

proper treatment is that which we adopt for pyrexial diseases in other parts of the body in the early stages, and is subsequently such as is of a tonic nature, and finally that which is calculated to remove chronic inflammatory thickening or the like. But there is something more than this to be considered. It is not enough to adopt a treatment recognised as suited to a particular disease. There are a number of influences that modify disease in different subjects, and that require to be carefully attended to. These influences are of very common operation, and arise out of the peculiarities of the patient. The dermatologist must determine not only what is best for individual cases, and how far the specific or formulated treatment for particular diseases should be added to, or modified, to suit these diseases in particular individuals. In fact, the treatment of the cases of skin diseases now under notice should be a combination of the remedies suited specially to the particular disease present, with those suited to the concomitants of the individual case. The following are the modifying influences to which attention should be given.

Firstly. It is important to recognise the great influence which scratching possesses in exaggerating certain diseases. Nothing is of more consequence than the use of special remedies calculated successfully to allay the irritation which forces the patient to scratch, and to protect and to soothe the parts when scratched.

Secondly. The influence of general debility is at work not only to retard recovery in, but to favour the development of many diseases to a more severe degree than would otherwise be the case. It is not a question of debility *producing* this or that malady, but *modifying* the same. The debility may express itself mainly in the form of anæmia, want of nerve power, &c.; and special and appropriate remedies given with a view to meet these several states will lead to an improvement in the general health, and thus indirectly favour the cure of any diseased condition about the skin; for it does not need any proof from me that the nearer a man approaches in his condition the standard of health, the sooner and more certainly will he throw off any disease about him, be it in his outer or inner surface or parts. But in practice we much forget that the specific should be linked with general treatment. And the relief of the debility of persons afflicted with diseases of the skin is to be accomplished not only by the use of medicines, but by mental and bodily rest, change of air and scene, and the like. These matters are by no means sufficiently considered.

Thirdly. Every attention must be paid to dyspepsia as affecting the origin and course of skin diseases. It gives rise, of course, indirectly to debility, to torpid action of the liver,



to the generation of acridities that float about in the blood, and circulate freely through the skin to disorder it; and, lastly, it increases cutaneous hyperæmia by reflex action. Dyspepsia must therefore be regarded as a powerful intensifier of all hyperæmic skin diseases.

Fourthly. The circulation of retained excreta is always a cause of intensification of hyperæmic conditions. The blood charged by excreta, in passing through an eczema or a psoriasis, will irritate it, if I may so say, and thus necessarily tend to give it an inflammatory character, whilst the process towards cure will be retarded. I believe this to be one of the most important of all points to be attended to in treating diseases of the skin, and the principle is of almost universal application. It is a matter of common sense (but matters of common sense are apt to be very much neglected) that a poisoned blood-current—poisoned, *i.e.*, “charged,” with bile or nitrogenous matters which we know must disorder the tissue nutrition and vascular tension—will not permit the cure to be accomplished so easily as if the blood were uncharged with the same elements; and the removal of the latter will help the cure of disease, and that oftentimes in a marvellous manner. Now, whence the excreta to charge the blood? From defective assimilation, as in dyspeptics, torpid liver action, and from excessive waste; or from defective excretion, by the skin, the kidneys, and the liver. The skin has to get rid of a certain amount of effete products; and if the skin acts sluggishly or scarcely at all, this may impurify to some degree the blood-current. So, again, deficient kidney action tells its own tale. The non-excretion of bile products in the usual way, and their passage into the blood-current to circulate within it, is another cause of skin mischief, as in some cases of erythema, urticaria, and probably purpura. The severest case of general eczema I ever saw was excited by the circulation of bile through the skin in connexion with a most marked attack of jaundice. Of course in such a case the influence of the retention of the non-eliminated products is readily appreciated, but in other cases readily overlooked. There are minor degrees of blood-impurification by bile, urea, and the like. It seems to me that the treatment of diseases of the skin is essentially the physician's work and not the surgeon's, for the very reason of the connexion between skin maladies and functional and structural changes in internal organs.

Fifthly. Deficient renal action, particularly in reference to the quantity of fluid discharged, is another matter of prime moment in regard to skin diseases. I am convinced that we do not, when the skin is disordered, sufficiently make use of the kidneys to relieve the skin of its work. There are three condi-

tions in connexion with diseases of the skin in which the kidneys should be freely stimulated to active excretion of fluid: (1) Where, in consequence of organic disease of the heart or other cause, and in connexion with a sluggish circulation, there is a distinct tendency to fluid accumulation in the tissues. (2) In all cases of hyperæmic skin disease, and in the early stages of inflammatory diseases, where the blood tends to accumulate in the skin in connexion with pyrexia. And (3) in diseases of the legs, where there is the slightest tendency to local or general œdema. In inflammatory diseases of the leg diuretics greatly counteract the injurious effects of mere gravitation, and they tend to relieve the engorged tissues. Diuretics are useful under all these circumstances, and emphatically so where the quantity of urine is already deficient. But I hold that in diseases of the skin generally a deficiency of urine calls for free stimulation of the kidneys to relieve the disordered skin of its work as much as possible. Of course where there is free circulation with the blood of retained excreta the necessity for free kidney action is increased tenfold. The liberal administration of diuretics in a case of eczema of the legs in a gouty subject will often act like magic when all other remedies fail.

Sixthly. Gout and rheumatism have their special influence upon skin diseases. I might have referred to these modifying influences under the last head, for it is the excess of uric acid in the one and the presence of lactic or an allied acid in the other case, and the circulation of blood charged with these products through the skin, that cause special mischief in skin diseases, *and give an inflammatory or irritable aspect to them.* The older practitioners, in attributing to gout and rheumatism the causation of disease which we now deem independent of them, were even nearer the truth than are some modern observers who hold excessive belief in the local origin of disease. If gout does not *produce*, it often *modifies* skin mischief, as above stated; and, after all, our forefathers were only at fault in regard to the use of terms. It is not an uncommon thing to have this or that eczema or psoriasis or lichen called gouty eczema, gouty psoriasis, gouty lichen. Practitioners have found that by treating patients affected by these forms of disease for gout or rheumatism, as the case may be, the maladies have often speedily vanished when other measures have failed. They have imagined that the gouty blood has caused the disease. It would be more correct to say it modified, it irritated, the disease, and so prevented the cure from taking place. With this explanation there can be no difficulty in seeing that gouty and rheumatic dispositions exaggerate hyperæmic conditions in skin diseases, and give an inflammatory aspect to them very frequently. The removal of gouty influences must aid the cure of skin diseases.



Seventhly. The tuberculous and the strumous diatheses also, it must be remembered, considerably modify certain skin diseases. The tendency in the phthisical and phthisically inclined and in the strumous is to a particular perversion of cell-growth in certain of the tissues, and notably the fibro-cellular: I refer to the tendency to the production of pus. Now it will be readily understood that this must have much influence upon local tissue-change in skin disease. Given the occurrence of a local inflammation in a non-strumous and a strumous subject, there will be differences in the two instances accounted for by the operation of the pus-producing tendency in the one and not in the other. It must be so. The tendency to the morbid cell-growth, which may not have shown itself in action before, will be likely to do so when nutrition is disturbed by local inflammation, disordered innervation, &c., as the case may be. The strumous tendency takes occasion by the perversion of local nutrition to give evidence of its presence and to complicate matters. Of this fact experience assures us. Take the case of an eczema: if it occurs in a strumous subject, it is accompanied by free pus-production. Acne in a non-strumous and acne in a strumous subject are different. In the latter case, the fibro-cellular textures of the wall and about the gland are implicated and inflamed, often presenting the appearance of the livid-red and the indolent tubercular swelling of a strumous tubercle, whilst the acne spots suppurate freely and leave behind scars, indicative of the tendency to ulceration, which is another feature of the strumous habit. This is a simple but it is an important point in therapeutics, and, because simple, often disregarded as of no moment. In psoriasis, in strumous subjects, the tendency to the production of pus is seen, and in this statement I am confirmed in my original opinion by my friend Dr. R. W. Taylor, of New York, an excellent observer and worker. Therefore, in regard to phthisical and strumous subjects, whilst one treats the local disease, be it eczema, erythema, lupus, acne, or psoriasis, it is of great use to neutralise, by the conjunction of anti-strumous remedies, the bias of the general nutrition towards free pus-production, which will of course express itself in proportion as the diathesis is marked and the local derangement of nutrition severe.

Eighthly. An old syphilitic taint is to be carefully detected and dealt with in reference to skin diseases. This should be recognised as a distinct and special cause of chronicity in certain non-syphilitic eruptions. The tendency of fully-developed syphilis in the human subject is to produce a new growth in the fibrous textures, or to give rise to a modification of the normal cell elements of these parts. The altered or new tissues we call "granulation tissue." Now, when

syphilis modifies ordinary skin eruptions, it does so by impressing upon them more or less of the characteristic tissue change it ordinarily produces, and in doing so may antagonise to some extent diseased processes peculiar to the disease which it modifies. Eczema may illustrate what I mean. The characteristic of eczema is the occurrence of serious catarrh in the papillary layer of the skin as the primary condition; but if eczema occur in a syphilitised subject, and the syphilis be active, then the tendency of the syphilis towards the formation of the granulation tissue may show itself, and the "serous effusion" may in part give place to the formation of new tissue—i.e. the two processes may be more or less intermingled, or rather, I should say, the result is a compromise. This is exactly what we should be led to expect from an examination of the naked-eye characters of eczema in a syphilitic subject. There is less discharge and more thickening than usual, and than we should be led to anticipate. So in psoriasis there is more thickening, less scaliness, and less vascularity, because the syphilitic tissue invades and presses upon the vessels in the normal tissue. Hence the effect of syphilis on ordinary skin diseases is to render them less typical, in regard to naked-eye characters and to their course, because the additional element of syphilis retards the cure, whilst there is the superaddition of the element of "thickening" or "deposit" (granulation tissue). Whenever I meet with a case of *unaccountable* chronicity I suspect that a syphilitic taint is in the back ground; and, for my own part, I believe that syphilis is a modifier of disease in local inflammations to a greater extent than has yet been expressed in any writings. Syphilis does not give evidence of its presence, as regards skin affections, until some strain is put upon the nutrition of the tissues, and then an explosion takes place. A strain is put upon the nutrition of the skin when a local inflammation occurs, its normal resistant power is lessened, and thereby syphilis, taking occasion by the weakness, shows itself in action as a modifier of the inflammatory process as described. A very familiar illustration of the fact that syphilitic complications or special development of latent syphilis in the system show themselves under such circumstances, is afforded by the outbreak of tubercular syphilis around a wound—about the shin, for example. What would be a simple wound in a healthy subject takes on unhealthy ulceration or syphilitic pustules, or tubercles develop about the seat of injury in subjects in whom syphilis is latent, but which had given no sign of its existence to the physician before. In like manner skin affections seem to excite latent syphilis at times into activity, and are thereby modified by it in the way I have indicated. I



know of no more important point than this in cutaneous therapeutics. If we treat the syphilitic taint we shall much more speedily and certainly cure the disease which is present.

“Chronic inflammatory thickening” is a matter upon which a few words must be said. In some cases this condition is found to be developed out of proportion to the degree of antecedent hyperæmia both as regards its severity and its duration. My own belief is that in such cases it is to be explained by the existence of the strumous diathesis in the individual concerned, which often at once accounts for the infiltration of the fibro-cellular tissue with a low type of lymph, or the formation in it of a new tissue of a less perfect kind. At other times an old syphilitic taint may best explain the occurrence of thickening. At all events a mercurial course sometimes does wonders, as we all know. Of course long-continued hyperæmia in a person free from the two diathetic tendencies named will lead to “chronic inflammatory thickening.” I am now particularly referring to cases in which thickening and induration either rapidly show themselves, or in such a manner as to strike us that it is due to some special tendency of the textures to hypertrophy rather than to the hyperæmia itself, which perhaps has been but slightly marked.

Ninthly. There is a class of phenomena connected with reflex action which it is of some moment to pay attention to in treating skin diseases. He who would deal with the matter scientifically must never concentrate his attention solely upon the mere local changes in these diseases, but must especially be on the alert to discover, in disturbance of the nervous system, the frequent reason of the intensification of hyperæmic conditions in the skin. I have seen not unfrequently a redness of the skin produced by mental excitement or sudden fright in “nervous subjects”; and there can be very few who have not known an eczema rubrum made immediately very much worse by the same influence. Depression and mental excitement play great parts in aggravating hyperæmic conditions of the skin. That uterine mischief occasionally intensifies acne rosacea, that dyspepsia frequently makes acne much worse, and the like, are facts belonging to the same category.

Tenthly. The hygienic surroundings of patients must always be carefully taken into account in the treatment of diseases of the skin, in reference especially to the class of cases particularly under consideration now. It is in this respect that the treatment of hospital and private patients of the better class differs so much. The confined living-rooms, the breathing of impure air, the defective diet, and the neglect of cleanliness, are influences that aggravate skin diseases in the poor as compared with the rich. It is cleanliness and feeding up, with

fresh air, that the poor want, in many cases of disease which would be relieved by purgatives and alkalies and nervine tonics amongst the better classes.

Now the several modifying influences referred to under the foregoing heads may be present in different combinations, and it is the full recognition of this fact that leads to success in the treatment of skin disease.

[In another number of the *Lancet* Dr. Fox refers to the hyperæmias, the simple inflammations in the debilitated, eczema, ecthyma, psoriasis, acne, pemphigus, &c.]

I am the advocate for a much more soothing system of treatment than that usually adopted for these diseases, of which hyperæmia is so frequently a part. And this leads to the question, What should be the object of our local measures? It is threefold: (1) at the outset, to *moderate* diseased, especially inflammatory, action; (2) to *protect* the diseased, and therefore weakened, parts; and at length (3) to *stimulate*, with the view of rousing the dormant tissues into due activity, and causing the removal of morbid depositions and formations. My own conviction is that, in the early stages of hyperæmic skin diseases, much of our current treatment is mischievous by reason of its activity. A soothing plan of treatment is wanted in all cases of early cutaneous congestions. By soothing treatment I mean one which diminishes congestion and secures an exclusion of air—one which, in fact, puts the skin in a state of rest. *Heroic* measures, designed to cut short an early congestive stage of a skin disease, often render the course of that disease chronic and persistent. In the early stages, before the deep vessels are involved, much can be done to check congestion by mild applications; whilst active measures do harm. For instance, in acute general psoriasis I have often seen aggravation of the congestion follow the use of tarry applications; while great relief has been produced by alkaline and bran baths, and subsequent oiling of the surface. I am not by any means an advocate of an expectant plan of treatment. I advise potent remedies to be employed after the congestive stages have passed. The dangers of over-stimulating are not imaginative; and they are plainly made apparent, in cases accompanied by active hyperæmia, by much irritability, or by a tendency to degenerative change in the skin, in the spread of the disease, and its undue chronicity.

I may refer to the local treatment of certain instances of lupus, *quoad* hyperæmia, in illustration of what I mean. Sometimes we have a patch of lupus on the face, which is very tender, very hyperæmic, and which exhibits a tendency to spread—that is to say, there is a tendency in the apparently



healthy tissues to become the seat of the cell-growth which is the characteristic of lupus. In these cases the exclusion of air and the use of mild non-irritating astringents will do good by diminishing the hyperæmia; whilst the use of caustics will be sure to cause the disease to spread, because it will greatly increase the hyperæmia of the parts around, and so favour the development of the peculiar cell-growth. In fact, if one still further disturbs the already weakened disease-tending tissue, one is, of course, likely to favour disease. This is also true of simple acne, pityriasis, rubra, &c.

Now for the details of local treatment.

First, as regards *moderating* inflammatory action. It will naturally occur to anyone first of all to remove special causes of local irritation in the case of hyperæmic skin affections. There are two sources of mischief I may specially refer to; they are (*a*) the wearing of flannel next the irritable skin, and (*b*) scratching. I think flannel should never come in contact with an irritable skin; it is a great source of irritation. It may be worn outside a linen garment, and the patient will not, under such circumstances, catch cold. The disuse of flannel is important in nettlerash, pruritus, eczema, scabies, the erythematæ. &c. Scratching does an infinity of mischief. The usual plan for preventing it is to use some sedative lotion. I think it of great importance to employ emollient and alkaline baths freely, to allay irritation in the early stages of the local inflammatory cutaneous diseases, and to subsequently protect the parts by appropriate coverings and applications; and by internal remedies remove all cause of pyrexial disturbance, or alter such blood impurifications as lead to an intensification of the hyperæmia of the parts through which the blood passes. The baths which are best adapted to moderate inflammatory or irritative action in the skin are bicarbonate of soda, 2 to 4 ounces, size from 4 to 6 or 8 lb., poppy and bran. The patient may remain in for ten minutes or so, and the skin should not be rubbed dry, but patted with hot towels. It is a good plan to oil the skin subsequently, or to powder it with oxide of zinc, or to apply a simple calamine lotion. But we must take care to use such means in due conjunction with general remedies. If bile products of ureal compounds are in abundance in the blood, free purgation, or diuretics employed with a liberal hand, must not be neglected. Pyrexia must be met with appropriate drugs and dieting. In like manner the pain and hyperæmia of an eczema in a gouty subject may be greatly moderated by an alkaline bath, but a good dose of colchicum in addition will bring the greatest relief. The exclusion of air from inflamed and hyperæmic irritable parts is a matter of great consequence, and the more so if the irritable part is de-

nuded of cuticle. The air is very stimulating to such parts; absorbent powders and neutral unguents are here called for. Then it is also very desirable to prevent evaporation taking place from scratched surfaces, or surfaces denuded of their natural protecting layer of healthy cuticle, for thereby they become harsh, hard, and tend to crack. I know nothing so good for the purpose as Kirkland's neutral cerate.

I don't know that I can find a better place to make one reference to the use of water-dressing to indurated and irritable parts, made all the more troublesome by scratching. There can be no doubt that the use of a water pad, which permits the diseased surface to absorb moisture and so become more or less soft and supple, does give great relief; but this is oftentimes succeeded by the recurrence of itching, pain, and, it may be, increased cracking and stiffness or tension. The explanation is not difficult to give in regard to some cases at least. The part has become denuded of its cuticular covering, and, whilst it absorbs moisture freely, it also parts with its moisture equally freely unless prevented. Whenever I use water dressing to an excoriated part, I invariably direct that a layer of greasy matter shall be applied, and kept applied, on the removal of the compress. In this way the absorbed moisture is retained, and the parts keep more or less soft.

But I am touching upon the second point—viz, the *protection* of hyperæmic and irritable parts; and, indeed, to *protect* is, under such circumstances, to *moderate* diseased action. To protect is to prevent scratching and the stimulation by the external air that gains access to the part and to secure rest, as it were, to the diseased parts; and to negative the operation of special external irritants. In addition to the use of absorbent powders and neutral unguents, one may employ cotton-wool, bandages, and strappings, which I need not now discuss at any length. The use of bandaging and strapping is particularly to be commended where the effects of gravitation are noticeable—in the legs, for example. The bandage prevents undue distension of the vessels and the escape of serosity into the textures, thereby giving the parts a better chance of recovering their tonicity. This simple matter is much neglected in regard to cutaneous diseases. Not only in the leg, however, but in other parts bandaging and strapping act similarly, where the tendency to engorgement of the tissues and vessels is shown.

The third object of local treatment in the diseases with which we are dealing is *stimulation*. One of the chief things the dermatologist should set himself to determine is the exact time at which his soothing kind of treatment should cease and his stimulating remedies be employed. It is at the moment that the irritability of the bloodvessels and the nerves is subsiding



that the use of powerful astringents and stimulants is so efficacious, and that it is possible to do the most towards a speedy cure of disease. What happens in the inflammatory eczema, the acne, or the psoriasis? After a while the vessels lose their tonicity and become dilated, favouring the "effusion of inflammatory products," giving rise to œdema, "chronic inflammatory thickening," &c. Now, it is at the moment that the parts are becoming less inflamed and irritable that astringents, lead, zinc, nitrate of silver, mild mercurial preparations, and the like, are of use to restore to the vessels their tone; and if at the same time our general remedies assist nature by removing impurities from the blood, by restoring the balance between absorption and excretion especially of the watery matter of the system, and giving general tone to the body, the patient will probably get rapidly well. The application of sulphur at the nick of time indicated speedily cures an acne that had been irritated before by the same remedy. The same may be said of the use of nitrate of silver to an eczematous patch. The use of tar to a psoriasis which is markedly hyperæmic at a very early stage will often spread the disease, or bring out fresh places. If matters still do not mend, but the disease holds on, one needs to stimulate, or, as we say, "rouse the torpid tissues to activity," but as I should put it, to quicken the activity of the absorbents in order that effused and formed products may be removed. It is really not so much in chronic inflammatory skin diseases that we need fresh remedies, as to use those we have reasonably, as regards time and circumstances. This is emphatically so as regards eczema and general psoriasis. But what are the indications that local soothing remedies are still needed in a disease, and the time has not come for stimulants? I think the main ones are as follows:—The tendency of the hyperæmia or the disease to spread; the development of new spots of disease, showing the disposition to the implication of healthy parts, which would be favoured in its development by anything which irritated such parts; the presence of much pain or heat in the parts; and manifestly the marked hyperæmia. But I lay most stress on the two former features. I like to let hyperæmic conditions in skin disease "quiesce," as it were, before I leave off my soothing treatment. But a disease having "quiesced," its hyperæmic condition having diminished, and the disease showing no tendency to spread or to spring up in new places—*e.g.*, acne, eczema, psoriasis, and pemphigus,—then stimulation can be appropriately adopted. And for what reason are stimulants used? First, to restore the tonicity of vessels; and one must not forget here that artificial pressure by bandages, &c., is a great assistance where gravitation comes into play to distend the vessels, as about the legs. Secondly,

to alter the character of a discharging surface. Such is the action of a weak mercurial ointment in a case of chronic eczema. Thirdly, to check the formation of scales, as in the use, against psoriasis, of tarry preparations; and, fourthly, to cause the absorption of inflammatory or heterologous formations; the iodides, mercurials, the soap treatment, blistering, and the like are specially referred to under this last head. But in regard to the use of stimulants and revulsives, I hold equally, as concerns their application, that their efficacy will depend greatly upon the judicious conjunction of internal remedies. I mean that, supposing that the blood of a patient in whom we use these external means is charged with uric acid, he will have a much smaller chance of gaining benefit from the application of revulsives than one who has a pure blood-supply passing through the diseased part. In the former the blood-state may take occasion, by the unbalancing of the nutrition induced by the use of the local remedies, to increase the hyperæmia, or to set agoing an inflammatory action again. The success of stimulant applications, in fact, will be in direct proportion to the degree in which the individual's condition approximates that of health. One very potent method of stimulating not only the skin but the system generally, and emunctory organs in particular, is the use of sulphur or sulphide of potassium baths. They should not be employed in the acute stages, but in the chronic forms of psoriasis, eczema, lichen, and acne. Where there is not much irritation or hyperæmia, and where the latter does not seem to be readily increased, they are most useful.—*Lancet*, June 1, and July 20, 1872, pp. 751, 71.

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#### 70.—SOME REMARKS ON THE PATHOLOGY AND TREATMENT OF ACNE.

By J. L. MILTON, Esq., Surgeon to St. John's Hospital for Diseases of the Skin, London.

Acne may be briefly defined as occlusion of, sometimes with increased secretion from, the ducts of certain sebaceous glands, attended by thickening, hardening, inflammation, or suppuration. Such pustules as occur mature very slowly. All the disorders usually classed under this name may be reduced to:—

1. Acne punctata (*amedones*, grubs), in which there is simply occlusion of the sebaceous ducts, with formation of black specks at the orifices, accompanied by elevation and hardening of the sides of the openings; contents composed of epithelial cells, oil-globules, some fine hairs, and cholesterine; may inflame and turn to acne simplex,—when hard and horny, often called stone-pock (*a. indurata*).
2. Acne simplex, in which this state is accompanied by formation of hard inflamed papules round



the orifices of the ducts, and frequently by suppuration. 3. *Acne rosacea*,—more an erythema, accompanied by papulation, than a pustular disease; distinguished by vivid red patches on face, sometimes with a dry and profavaceous condition, at others by a greasy state of the skin, though, indeed, some degree of seborrhœa may attend every variety of acne; suppuration occasional; pustules often painful and stiff; occasionally varicose dilation of minute superficial veins. Hypertrophic acne, in which the skin is thickened and rough, with red and purplish knobs, greasy and shining, is an aggravated form of this variety.

In acne simplex, as generally met with, it will be observed, on examining the surface carefully with a good lens, that in some of the spots inflammation has occurred round the orifices of the sebaceous ducts, on the apices of which are seated small brownish adherent crusts. At these places the hair-sac appears to have been torn out by scratching, or to be imbedded in the crust. In others, where this process has not gone so far, we only see the firmly-occluded hair-sac with its blackened orifice. I have never seen a hair issuing from one of these sacs. As very often no sebaceous matter can be squeezed out of these places, I am disposed to think that the irritation set up is not due to the imprisonment of the sebaceous matter, but to a morbid action in the hair-sac itself.

Acne is one of those diseases which demonstrate the inherent weakness of any classification. Although it is in one form—acne simplex—essentially a pustular disease, and frequently so in another variety—acne rosacea; yet suppuration, rare in the latter, is sometimes unknown through the whole duration of a case of the punctate variety. This is, however, no more than we find in other groups. Ecthyma is at times a vesicular disease; at others, it forms by a process very closely allied to that seen in disorders called sometimes impetigo, and, again, impetiginoid eczema. Further illustrations of this unsatisfactory state may be found in the separation of smallpox from the vesiculæ, though, so far as I can see, it is as essentially vesicular as herpes; and in the connexion of tinea circinata and pityriasis, which may pass into eczema with tinea tonsurans and pityriasis versicolor, which do not undergo this change.

*Treatment.*—The slighter cases of acne, and of the disorder known as steorrhœa, require little more than using no soap, and washing in hot water. If somewhat more severe, the spots may be bathed freely with a weak goulard or bismuth lotion.

But more advanced cases naturally require more energetic measures; and in order to make perfectly clear what I have to say, I propose to divide the treatment into four stages, em-

bracing four groups of remedies. These are:—1. Preparatory treatment, especially called for when the tongue is foul, the appetite bad, the health out of order, and the bowels constipated. 2. Treatment devoted to the restoration of the strength and nutrition. 3. A course of remedies directed towards the absorption of material deposited in the indurated parts. 4. Remedies calculated to act peculiarly on the skin.

1. The first set of symptoms requires, in my opinion, saline aperients, with or without bitters, according to circumstances. Sulphite of soda or magnesia, or the heavy calcined magnesia, may be given with nitrate of potass in mint water, or, when the appetite is bad, in some bitter aromatic infusion, as calumba or serpentaria. However slight may be the signs of improvement which follow the use of these medicines, I feel little hesitation in recommending their steady continuance along with some mild aperient pill. I am the more particular in saying this, because I believe that, in the treatment of skin diseases, the rock always ahead is the slow progress made at first, and that it is most necessary, when we have once traced out mentally the path to be taken, to keep rigidly to it, unless, indeed, some quite unforeseen contingency should arise. At the same time, it is to be quite understood that bad cases are rarely cured by such means, the principal value of which consists in clearing the way for further measures.

2. After salines and aperients I would advise a course of steel. Perhaps the compound aromatic steel mixture, or the tincture of the sesquichloride (Pharma. Lond.) are as good as any for this purpose, and a full dose of one of these—say half a drachm to a drachm of the tincture, or an ounce of the mixture—may be given two or three times daily for six or eight weeks. I have, in some instances, both in these forms and in acne rosacea, observed that the sulphate of iron agrees better than the tincture, and in women of middle or mature age I have seen it effect a rapid improvement, especially in rosacea, after iodide of potassium, liquor potassæ, Donovan's solution, tonics, and aperients had been tried in vain. If any constipation arise from the use of iron, an aperient may be given. For this to be effectual, three ingredients should be present. First, aloes in the form of aloin, or some preparation which will not irritate the lower part of the great gut; secondly, a minute quantity of mercury, so as to act gently on the liver; and, thirdly, a powerful aromatic, such as myrrh, sagapenum, or assafoetida; an aperient extract or pill may generally be added with advantage. Dr. Fraser says that when emmenagogues are required, the oil of savin in doses of one drop three times a day is a very valuable remedy in this eruption.

3. But in cases where there is no particular weakness or un-



healthy state of the system, no chlorosis or defective menstruation, where dyspepsia or acidity of the stomach prevails, where steel induces acidity, headache, giddiness, and pain in the bowels, and when the acne is not benefited by the foregoing medicines, I think it will generally be found that an alkali is more suited to the case. Perhaps it would be difficult to select a better preparation than liquor potassæ in doses of thirty to forty minims twice a day in a tumbler of milk. The bicarbonate of potass has been recommended for this purpose in punctiform acne. Dr. Hillier used to give the liquor in combination with hydrocyanic acid and a vegetable bitter. As an aperient, calcined magnesia may be prescribed along with the potass; about half a teaspoonful to a teaspoonful of the heavy calcined is generally enough; it also may be taken in milk before breakfast. Generally it acts more satisfactorily if preceded by an aperient pill overnight.

4. The probability, however, is, that after effecting a certain amount of improvement, the foregoing remedies will cease to exert any control over the disorder. So soon as ever this becomes manifest, I would advise that arsenic in the form of Fowler's solution, without the spirit of lavender, should be added to the liquor potassæ. I have found the combination extremely beneficial in scores of cases. A young person of eighteen or nineteen, or an adult, may begin with thirty-minim doses of the potass and five minim of the liquor arsenicalis three times a day, taking them in milk, and immediately after meals. Younger and delicate persons should try smaller doses. After a time the quantity of arsenic should be increased, but acne seldom seems to require larger doses of this drug. So soon as decided improvement sets in, the potass may be given up and the arsenic alone continued. While giving either, however, five grains of the compound calomel pill may be advantageously prescribed occasionally at bedtime. When expense is no object, the arsenic may be taken in compound decoction of sarsaparilla; but, to have any effect, the dose must be quite an ounce to two ounces, and the decoction must be good. I have had ample opportunities of satisfying myself that what is constantly sold as some unfailing preparation of sarsaparilla, exerts no control over acne, and most probably contains little more than a mere flavouring of this drug. It is no uncommon thing for hospital patients, especially for those whose material interests are likely to be seriously affected by the presence of such a disease—as in the case of men-servants, for instance—to spend almost their last shilling on sarsaparilla before applying at a public institution for relief, and that without deriving any benefit whatever.

Mr. Hunt seems to think time and money are simply wasted

in trying any remedy except arsenic. I have given his plan a fair trial, and feel no hesitation in recommending as superior, that which I have laid down. My experience is that the patient bears arsenic always as well, and generally a great deal better, after a preliminary course of salines, acids, steel, and potass. Besides, arsenic will not set right a coated tongue, loss of appetite, sluggish liver, &c., symptoms of which many persons, as previously observed, complain at the outset.

Some writers have thought very highly of acids (as the great basis of treatment, I mean), sulphur, and antimony, in acne. The two latter I tried pretty extensively at one time, but they appeared to me inert. Possibly, taken in some warm drink on going to bed, they will bring on perspiration, and thus achieve a certain amount of success in the minds of those who think there is some mysterious connexion between the suppression of this function and the breaking out of acne. Tar and charcoal, taken together in the form of pills, seem, according to a communication made to me by Mr. John Wetherfield, to have answered very well in acne punctata. The most extraordinary success, however, that I have yet heard of occurred in the practice of M. Gübler, from the use of glycerine. He gave two dessertspoonfuls daily in acne punctata (acne simplex), and the pustules began to diminish in number and volume "from the day that the remedy was first taken.

I need scarcely say that numberless external remedies have been recommended in acne. I have no doubt that the accounts of the success said to have been attained by the use of these are all in good faith; but, at the same time, I feel bound to say that my own observations have led me to conclusions very unfavourable as to their efficacy; indeed, I am disposed to believe that few local remedies possess any power over this disease, and that the control exerted by those is limited and uncertain. The best of these seem to be preparations of chlorine and iodine, with sulphur or mercury as a base. It will be found that they all possess a certain amount of epispastic power; and it is probably to this that they owe their beneficial action, it being, so far as I can see my way, very doubtful whether they, one and all, equal a blister in efficacy. Foremost among these preparations stands the hypochloride of sulphur, with which the surgeon may begin at once. At one time I used to prelude the employment of it by ordering, first of all, the ointment of the subchloride of mercury, and then of the ammonio-chloride, but after watching their effects in a great number of cases, I have come to the belief that the former, though recommended for skin diseases generally by so high an authority as the late Dr. Pereira, is almost, if not quite, useless in acne, and the ointment of ammoniated mercury not very reliable.



The ointment of the hypochloride should be smeared pretty thickly on the spots at bedtime, and allowed to remain on all night. In the morning it may be washed off with very hot water and pure soap. When the patient can afford it, glycerine should be added to the water. In some cases a weak lotion of bismuth or borax does good, and as both are harmless, both may be tried. I know of no rule to discriminate at the outset those cases in which they are likely to succeed.

When the hypochloride begins to lose its effect, biniodide of mercury ointment—15 or 20 grains of the salt to an ounce of lard—may be substituted. A very small quantity should be worked into each spot with the point of a butcher's skewer or an ivory stilet. If there be any large very indurated spots, it is a good plan to pierce them with a trochar not thicker than a stout sewing-needle. The bleeding spots should be well bathed with hot water. This is easily done by means of a flannel pad and a small mandril. The pad is dipped in almost boiling water, shaken, and pressed against the spots. A blister acts still more efficaciously in clearing the way for the iodide; but then it is almost impossible to get patients to blister. I have not succeeded in doing so more than three or four times. When the patient can manage to take them, I always recommend a course of vapour baths towards the close of treatment.

Rosacea may be fairly cited as one of the diseases which most strikingly point out the great improvement made of late years in the treatment of cutaneous affections. It is not so long since Bateman's time, yet then rosacea was universally thought incurable. Bateman himself maintained that a perfect cure could seldom be accomplished, and this belief is still only too prevalent. M. Bazin, writing in 1860, says that the arthritic form (*couperose arthritique*), "*est tres tenace, et fait le désespoir du malade et du médecin.*" Yet rosacea is not so unmanageable. In general it is much more curable than acne; and it may be safely said that the difficulty lies rather with the patient than with the complaint. Indeed, I believe there are few severe diseases of the skin in which the progress of treatment can be more accurately calculated, and a favourable prognosis given with greater confidence, than rosacea.

The treatment need not differ from that pointed out for the other varieties, except that in rosacea it is often quite unnecessary to give more than the salines and a short course of steel or alkalies. Arsenic is rarely called for. The external treatment is in every way identical.

Suppose the medical attendant has tried all these means in an obstinate case of acne, are there any others on which he can rely? I am afraid the question must be answered dubiously; there are, no doubt, remedies enough; but when we scrutinize

the proofs on which their reputation is based, we find little to encourage us in the belief that they possess any power of curing the disease superior to that of the remedies already mentioned. —*Edinburgh Medical Journal*, July 1872, p. 27.

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#### 71.—THE TREATMENT OF ULCERS OF THE LEG.

[The Editor of the *British Medical Journal* gives a report on the mode of treating ulcers of the leg at some of the principal hospitals in London. Mr. Barnard Holt's plan of treatment, at the Westminster Hospital, appears to us the most deserving of notice. At nearly all the other hospitals skin-grafting is the treatment adopted.]

Mr. BARNARD HOLT's treatment for ulcers of the leg consists in an endeavour, by proper position, attention to the general health, and suitable dressings, to procure a moderately healthy granulating surface, taking care to remove with the knife all hypertrophied cuticle in the margin of the ulcer. Having done this, provided the ulcer is not very extensive, he invariably hermetically seals the wound by the application of oiled silk, collodion, and strapping in the following manner. A square piece of soap strapping two inches larger than the outer circumference of the ulcer, having a hole made of the exact shape of the ulcer, is applied upon the leg. Upon the strapping good collodion is applied with a brush, and over ulcer and strapping one square piece of oiled silk is laid. This at once seals the ulcer; and, in order to prevent the edges of the oiled silk from rubbing up, he further fastens them down with small strips of plaster. If the discharge be profuse the sealing will require to be repeated in a couple of days, but the intervals of removal are quickly prolonged, inasmuch as the exclusion of air limits the discharge and facilitates the formation of skin from the edge of the ulcer. No other application is required, and the ulcer speedily heals. Mr. Holt never destroys exuberant granulations by sulphate of copper or other caustics, as the encroaching skin speedily produces their absorption, and the application of caustics destroys the fine filamentous cuticle that is to close the ulcer. He has employed this method of treatment for years with the most satisfactory results. Of course, these observations only apply to ulcers that are not the result of any specific poison.—*British Medical Journal*, June 15, 1872, p. 642.

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#### 72.—REMOVAL OF CORNS.

Hard corns may be carefully picked out by the aid of a small sharp-pointed scalpel or tenotomy knife, and if well done the cure is often radical, always perfect for the time. But they



may be equally successfully removed by wearing over them for a few days a small plaster made by melting a piece of stick diachylon (emplastrum plumbi), and dropping it on a piece of white silk. The corn gradually loosens from the subjacent healthy skin, and can be readily pulled or picked out. Soft corns require the use of astringents, such as alum dissolved in white of egg, or the careful application of tincture of iodine. Prevention, however, is in regard to them better than cure, and can be readily attained by daily friction with cold water between the toes.—*Edinburgh Medical Journal*, June 1872, p. 1136.

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### 73.—INGROWING NAIL.

By BENJAMIN BLOWER, Esq., Liverpool.

I desire to add a mite to the evidence repeatedly given in the *Journal*, that the removal of the nail (to my knowledge not always successful) is unnecessary.

About twenty years ago, I applied a bit of compressed sponge to afford temporary relief, and was delighted to find that it effected a radical cure. I make the sponge as solid as leather, by wetting and then winding string very tightly round it, and drying it thoroughly. Of this I cut a small pyramidal piece, less than a grain of rice. This I insert beneath the nail, and secure it by strips of adhesive plaster, applied longitudinally, to avoid compression. The sponge soon becomes moist and swollen, keeping the nail from the irritated flesh. Any granulations should previously be destroyed with strong nitric acid. I have adopted this plan upon many occasions, and have never found it to fail.—*Brit. Med. Journal*, Sept. 21, 1872, p. 327.

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## SYPHILITIC AFFECTIONS.

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### 74.—ON SYPHILIS.

By M. RICORD, Paris.

[The following is an epitome of M. Ricord's speech before the Surgical Section of the British Medical Association, when requested to explain his views on that subject, at the conclusion of a paper by Mr. Acton.]

There was one great question in regard to syphilis, and it was this: could it be cured radically? In former times all venereal affections, no matter what, were considered as belonging to syphilis, and certainly there was then an immense number of radical cures by mercury or any other means. In this way swellings of the glands, soft chancres, even warts, and other

things not belonging to syphilis, were easily enough cured; radically cured; and there were no after-consequences, no secondary symptoms. This explanation would account for the immensely large number of cases of (reputed) syphilis which used to be radically cured. But, since syphilis had been correctly diagnosed, the inquiry to which he had devoted a large part of his life was to see what belonged to syphilis, and what resembled it without belonging to it. There had been great differences in the results of treatment—so much so that a doubt, as Mr. Acton had said, had arisen whether real syphilis could be cured. That doubt as to the curability of syphilis was not recent; it was a doubt which old authors had expressed; and one particularly, with a curious name, which they would probably remember—"Mercurialis"—thought that now and then an armistice might probably be made with syphilis, but that there was no real cure. In fact, they frequently saw that a long time—months, years—after the symptoms had been treated new symptoms appeared. And so the doubt whether syphilis could be radically cured, or whether the cure was only temporary, with a prospect of the symptoms returning, might still remain; he (Ricord), however, had established the law of the unicity of the diathesis of syphilis. The law of syphilis was the same as the law of small-pox, cow-pox, or measles. A man could have but one attack so long as the disease remained in the constitution—that was to say, according to his opinion a new attack could not take place while the system was still under the influence of the old diathesis. Well, it was exactly so with syphilis; as long as a patient was labouring under the diathesis of syphilis, another *infection* of syphilis could not occur—it was impossible. For instance, after indurated chancre, and the appearance of secondary symptoms, it was not possible for the patient to contract a new indurated chancre, with swelling of the glands, manifestation of skin disease, and so on. After one attack the patient could not have another infection as long as the influence of the first remained in his body; a second contagion could not take possession of the system at the same time. If, perchance, something of the kind took place, the symptoms would not follow the regular evolution. So, when a patient had constitutional syphilis, if a new chancre appeared to be hardened they would not find the glands swell, or the early manifestation of skin disease appear; and so of other symptoms. Superficial ulceration might take place, just as a spurious form of vaccination might arise on one who was still under the vaccine influence; but it was not a true case, it was not attended with the sequelæ. But if the constitutional disease were cured, if the syphilitic disposition were completely eradicated, then the patient would be able to contract a fresh



indurated chancre, with all the subsequent symptoms. If this were the case—and he had observed it with great care, his experience dating back forty years—it proved that syphilis could be cured; and if syphilis could be eradicated, to ascertain whether a patient was cured or not when all the symptoms had disappeared there would be nothing else to do (though he knew that could not be done) but to try inoculation from an indurated chancre. If vaccination did not take, they were sure the vaccine disposition continued; if it did not continue, vaccination could take effect. In regard to syphilis, the proof had not been carried to this extent; but he had been able to observe that as long as the syphilitic influence continued, a patient could not contract an indurated chancre anew, and that, consequently, if cured, a new infection might take place. This was a great point gained in science, and it proved what he had said, that syphilis could be radically cured. Now, as to the treatment of the disease. As he had told them, Mr. Acton's ideas were completely his ideas, explaining his manner of treatment and his practice. He would first speak of the treatment of the first stage—that was to say, the primary sore. As soon as he had ascertained that there was a hardened chancre, with a swelling of the glands—not inflammatory, because the glands in this case never suppurated,—he immediately instituted the mercurial treatment. There was one point on which there was some difference of opinion: many believed that it was impossible to prevent the accession of the secondary symptoms, the first manifestation of constitutional disease; many thought that no matter what treatment was employed the sequelæ would appear. Well, he had ascertained that if the treatment were soon begun and well carried through, the bursting out of the first secondary symptoms, the roseola, the swelling of the glands of the neck, &c., might be prevented. If this were not frequently the case it was because the treatment was resorted to too late, when the disease had had time to take root, and secondary symptoms were about to show themselves. In such cases it was not astonishing that secondary symptoms should appear, and the treatment ought not to be blamed; if the treatment were steadily continued they soon disappeared. But if the treatment were begun early, the observation of forty years gave him the assurance that secondary symptoms would not appear. When secondary symptoms had appeared the best treatment was, as Mr. Acton had said, mercury. If they wished for a perfect cure, this treatment must be continued. In general it was not persisted in long enough, it was dropped as soon as the symptoms disappeared, or a short time after, and then it was not astonishing to see them reappear. But if the treatment were continued five or six months, having regard at

the same time to sustaining the constitution in general, relapses would be found to be infrequent. He observed very few cases of relapse, and there would not be many when the treatment was well kept up—when the patient had patience enough, and the physician sufficient courage. After six months of that treatment and no symptoms re-appearing, then the treatment with iodine must be begun, and continued for five or six months more. When a patient went to him, he said, “You will have a year’s treatment—do you consent to that?” “Yes.” “Very well; we shall go on. If not, Good bye.” There were cases in which syphilis occurred in a healthy person—the only disease was syphilis. Then treatment was very easy—the case was a simple one; they had but one enemy to fight—all went on regularly. But, unhappily, in many instances syphilis was not alone; there was something else—scrofula, skin disease, scurvy, low constitution, poorness of the blood. They must understand that such complications as these altered the case; the treatment did not act so powerfully as it would do in the first case, as many of these complications were aggravated by the treatment. For instance, syphilis and scurvy might coexist—and the characteristic of the latter was poorness of the blood, while that of the former was a plastic condition of the blood. Here, therefore, was a counteracting influence to the treatment of syphilis. Now, one thing must be known. In many instances syphilis became the secondary consideration, and they must begin with the constitution of the patient, as debility was the disease that required first treatment. They must attack the strongest enemy first. Syphilis was sometimes quiet, and stopped and waited till they came to it. So, when they had improved the constitution, they might commence with the treatment, and they must begin by treating the constitutional complication. The best treatment was the proto-ioduret of mercury. The stomach bore this well in general. Sometimes it gave rise to a little diarrhœa, which was an easy thing to moderate; but when the stomach was not tolerant of the remedy, one capital treatment was that which Mr. Acton had told them he had confidence in—namely, rubbing-in. If this were not an unpleasant and disagreeable operation, certainly it would be in general about the best; he himself should prefer it. In rubbing-in, the action of the remedy was powerful and quick, and the stomach was not at all troubled with it. If it were not so disagreeable, and were a thing that could be done without being noticed, he should give it the preference. However, there were cases in which the skin was otherwise affected, in which there was a skin disease, and then friction could not be used. In a case of complication of syphilis and herpes rubbing-in could not be resorted to. In general, patients bore the



iodide of potassium well, and in large doses. For his own part he frequently employed forty, sixty, eighty, even a hundred grains a day, and more. They must bear in mind that if they gave too small doses to some patients they would have no result; it was a remedy that passed through the body with great rapidity. He had had great experience of it, and he had found that in half an hour it had passed away in the urine. Iodide of potassium was a sort of broom of the blood. So they saw that the methodical treatment was this: mercury, iodide of potassium. But only one for the first stage, and only the other for the later stage of syphilis? No, the rule was absolute that as long as there were secondary symptoms well marked, mercury must be given; when there was a mixture of secondary and tertiary symptoms, mercury and iodide; for tertiary symptoms, iodide. To treat some patients with iodide would not advance them in any way. Why? Because there was frequently in the constitution, in the blood, something of the second stage, something that required the mercurial treatment. This might not show itself, but when iodide of potassium ceased to do good, the disease remaining stationary, let them go back to mercury again, and they would have a splendid result where they had thought there was no further possibility of curing the patient. This was what Mr. Acton had said, and he was completely and absolutely of Mr. Acton's opinion. But there was another thing. When syphilis had lasted for a long time, and had had a great effect on the constitution, it in some way disappeared, and left the patient with a complication existing that was not existing before. Sometimes a long course of treatment brought on a new disease—wasting of the constitution, poorness of blood. They must then stop all the specific treatment, and, applying themselves to the principal symptom, restore the constitution by preparations of iron, bark, tonics, and proper food, so bringing the patient back to the possibility of undergoing anew a regular methodical treatment, either by mercury or iodide, or a combination of these two remedies. In former times, when a person was thought to be syphilitic, physicians seemed unable to entertain any other idea than that of syphilis, and acted exclusively against a specific disease, neglected everything else, and in that way they experienced all the bad effects and accidental symptoms which a bad administration of the symptoms would produce. Mr. Acton had spoken of the use of bromide of potassium. His views were exactly the same as Mr. Acton's with respect to the use of the remedies at different stages, the necessity of having regard to the complications that might exist, and of dropping the treatment for a while till the constitution was restored. This was regular and methodical, and his own manner of practice. But

now, was bromide of potassium an anti-syphilitic remedy? He did not believe that it was. He might be mistaken; but he had experimented with it in syphilitic symptoms, and without any apparent result. But it was a splendid remedy in complications of syphilis. In some cases of symptoms referable to the nervous centres, bromide of potassium was an adjunct, and came to the help of mercury or the treatment by iodine. In some cases of brain disease with syphilis, and of disease of the spine or epilepsy, bromide of potassium did wonders. So that they would see it was a remedy to be applied in nervous complications that might occur, but they must not depend on it as an anti-syphilitic remedy. Now, there were symptoms following syphilis which were not syphilitic, and these must not be treated with mercury or iodide of potassium. For instance, there might be necrosis. Well, they could not bring a dead bone back to life, no matter what quantity of mercury or iodide of potassium they might give. A physician must know these things, and he (M. Ricord) ought almost to apologise for bringing them forward. It should be observed that specific remedies did not always act specifically. Certainly, there was no specific effect without a specific cause, but specific causes did not always act specifically. So there were some effects of syphilis, such as disease of the bones, that would afterwards act as a common irritant. In syphilis there might be an ulcerated bone in the nose or mouth, bringing on suppuration; mercury or potassium would not remove that, but let the diseased bone be removed, and the patient was frequently cured. They must take note of all these conditions—the nature of syphilis, the manner in which it conducted itself, its action on the constitution. Let them particularly take note that the general law of syphilis was the same as the general law of small-pox, vaccine, and measles. If they were sure of this from what he had said and from their own experience, then they might be sure that syphilis could be perfectly, radically cured. They could tell their patients that, and give them courage and hope. If the patient had courage to go through with the treatment, and the physician had courage enough to stick to it, the patient might be radically cured. He thanked them for the reception they had given him; it reminded him a little of his hospital in Paris.

A question was asked whether Dr. Ricord was a believer in salivation.

Dr. Ricord replied—No, surely not. Salivation was an accident following the treatment, and it must be avoided as much as possible. There was but one case in which he approved of salivation, and that was in disease of the eye—iritis. When this occurred, and salivation was brought on, the inflammation of the iris subsided.—*Lancet*, Aug. 17, 1872, p. 228.



# MIDWIFERY,

## AND THE DISEASES OF WOMEN AND CHILDREN.

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### 75.—A NEW USE OF BARNES'S DILATORS.

By Dr. HERBERT M. MORGAN, Lichfield.

As I have on two occasions lately found Barnes's dilating bags of great service in what I believe to be a new way of using them, I shall be glad if publicity can be given to the following narratives, as it may be the means of helping others in similar cases.

*Case 1.*—A woman, who had had a tedious labour as a primipara, was taken in labour with her second child, and the "waters broke" very early. I found the pains moderately good, but the os small and unyielding. The natural and elastic dilator of the os—viz., the bag of waters—had gone; and here was a case which in the ordinary way would be very tedious to the patient and her attendants. I thought that, if I could make an artificial bag of waters, so as to imitate nature, I should materially assist the labour; so, with great difficulty, I managed to introduce Barnes's largest bag within the os by the help of an uterine sound. When once it was there, it was easy to pump in nearly a pint of warm water; and I was much gratified to find that the labour then progressed very well, each pain dilating the os by means of the artificial bag quite in a natural way. I am sure that by this means I gained some hours, which would have been lost in useless pain but for the bag.

*Case 2.*—A farmer's wife had a miscarriage at about the eighth month of her fourth pregnancy. This was followed by a discharge of more or less blood for about a week, when suddenly she had such a copious loss as to make her quite faint. She then first sent for me, and I found her with a blanched face and constant sickness. The os was dilated to the size of a florin, and not yielding; the membranes were entire; and I could detect that it was a case of placenta prævia, with the cord attached just inside the os. I ruptured the membranes with a stilette; and then, by means of a long pair of ovum-forceps, I passed Barnes's largest bag (rolled up small) quite into the womb; and afterwards pumped into the bag nearly a

pint of warm water. As she had no pain worth mentioning, I had given her forty minims of liquid extract of ergot before puncturing the membranes. As soon as I had filled the bag with water, I commenced dilating the os myself by drawing at the tube of the bag till my finger and thumb could reach the root of the tube in the vagina. If I had pulled at the tube, it would have stretched, rather than allow the bag to dilate the os; so I took hold of it just where it joins the bag (which will be noticed as being the strongest part), and kept steadily pulling at it. The result was that, in a little more than five minutes, I pulled a soft bag, as large as an ordinary new-born child's head, through the os; and then the vagina and perinæum were gradually dilated in the same way until the bag came quite away. I found the head presenting, and pains were coming on moderately; but, as there was no time to be lost, I preferred not to wait for nature to act; so, with one hand internally, and the other externally, I turned the child and brought a foot down, and soon completed the labour with the aid of the patient's own pains and efforts. The placenta was expelled naturally, and the womb contracted well after it.

Now for the advantages of this mode. When I arrived, I found the os so unyielding, that I feel sure that the ordinary mode of dilating by the hand, in form of a cone passed into the vagina, would have been a very doubtful process. It might have failed from cramping of the hand in the attempt; or, worse still, the os might have given way, and the uterus been ruptured. Any way, dilatation could not possibly have been accomplished so quickly, so easily, so painlessly, and so safely, by the hand. Moreover, this bag, by being pressed against the bleeding placental vessels in its passage through the os, compressed them and checked the hemorrhage, in the same way as the head does in these cases where the pains are strong enough to keep it well pressed against the os.

I may mention, in passing, that, before using the bags, it is well to try how many syringes full of air are wanted to bring them to the required size. This is a great help, as they cannot be seen when in use.

I am not aware that this method of making a new bag of waters has been published before; but I have no wish to claim priority, if any one has already written about it. I shall be quite content with adding a little extra testimony to its value.—*British Medical Journal*, May 18, 1872, p. 523.

[We may mention that, at the meeting at Birmingham of the British Medical Association, Dr. Morgan showed us an improvement on Barnes's dilators, for the purpose named in this article. The bag now recommended by him is pear-shaped, and considerably larger and stronger than Barnes's largest



sized dilator. An unyielding material was also inserted at the junction of the tube, in the substance of the india-rubber, and this material extended a little upwards on the ball and several inches down the tube. The object of this is to strengthen the point of junction and the upper portion of the tube, in order that traction may be made upon the base by means of the tube. This is a great advantage, as the dilatation of the os can thus be much facilitated owing to the shape of the india-rubber ball. We consider this improvement of Dr. Morgan's, one of great value. The dilator was made by Weiss.—EDS.]

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76.—ON THE EXTERNAL AND INTERNAL ORIFICES OF  
THE UTERUS: THEIR ANATOMY, PHYSIOLOGY,  
AND PATHOLOGY.

By Dr. J. HENRY BENNET, late Obstetric Physician to the  
Royal Free Hospital.

Anatomically, there are two distinct cavities in the uterus; that of the body, the larger one; that of the cervix, the smaller one. It is in the cavity of the body of the uterus that conception and the development of the foetus take place, whilst the cavity of the cervix is a species of antechamber which precedes and protects the more sacred internal precincts. The circular fibres of the body of the uterus converge towards the cervix, are continued throughout its entire extent, and form the greater part of its tissue, half an inch in thickness. These circular fibres of the cervix are endowed with vitality, and with contractility in the cervical canal, even in the non-pregnant state as is seen on introducing a stick of nitrate of silver, or a solution of nitrate of silver, into the cervical cavity. Its entire extent instantly contracts, so that the uterine sound can no longer be introduced without an effort, although the canal may have been quite patulous before. These circular fibres, in the healthy uterus, surround and form an infundibuliform cavity in the central cervical region; whilst at the outer or vaginal outlet, and also where the cervix passes into the uterine cavity, they are in circular approximation, like the sphincter ani. They thus close the canal in both directions, and resist the effort to enter either cavity with sound or bougie, and that by a vital contraction. In a word, the cavity of the uterus is separated from the cavity of the cervix by a regular sphincter, a fact which I was the first to demonstrate in 1848; and the cavity of the cervical canal is separated from the vagina by a second sphincter. I do not mean to assert that on dissection a separate band of muscular fibres, assuming the form of a sphincter, can be demonstrated, nor have I ever made such a statement. What I have asserted and do assert is, that the circular fibres

which constitute the mass of the thick structure of the cervix uteri, or some of them, contract vitally and perform the functions of sphincters. I would remark that, anatomically, all the great cavities of the body have sphincters—the stomach, the intestinal canal, the bladder. For the uterus not to have one, would be an exception to this anatomical rule.

Physiologically, these sphincters open and close under physiological conditions. Before, during, and after menstruation, the os internum often opens to gentle pressure in cases in which it is impassible, except to force, in the interval of menstruation. In a much minor degree I notice the same fact with the healthy os externum. In all probability they both open, likewise, under the influence of sexual orgasm, thus facilitating the entrance of semen and conception.

On the other hand, the os internum constantly closes spasmodically on the contact of the cold uterine sound, so as to require more or less force for it to pass, and that in the healthiest female. A warm wax bougie of the same size will, however, often pass with gentle continued pressure. The contact of any astringent or irritating substance—alum, zinc, tannin, nitrate of silver—occasions, in a few seconds, such a contraction of the os externum as to effectually close the canal. Closure of the uterine orifices, indeed is so much the rule in the healthy female, that an open patulous condition of either orifice may be considered to generally imply disease, inflammation, inflammatory hypertrophy, or a fibrous growth.

To me it appears that the above statements are facts, undeniable, uncontrovertible facts; and if so, it is of the most vital importance that they should be generally recognised. In actual practice, we constantly hear of serious operations performed on the uterus by eminent obstetricians or surgeons, founded on the non-recognition of these anatomical and physiological data. Such operations are based on the supposed existence of strictures, which, if I am anatomically right, often do not exist. A remarkable fact connected with the operations the abuse of which I which I criticise is, that one class of eminent practitioners, such as Sir James Simpson and Dr. Greenhalgh, very frequently divide the os uteri internum as the seat of stricture, and as the cause of uterine suffering of all kinds; whereas other practitioners equally eminent, Dr. Marion Sims and Dr. Emmet, for instance, I believe, think that the os internum proper is seldom in fault, and divide the os externum down to its vaginal attachments in all but exactly the same class of cases. Thus, the practice of each category stands condemned by that of the other.

The division of the os uteri internum for sterility, dysmenorrhœa, &c., was introduced by the late Sir James Simpson



more than a quarter of a century ago, and I do not think that, up to the end of his career, he ever discriminated between physiological and morbid contraction of the os internum; or, in other words, that he ever adopted the above view long ago submitted by me to the profession. I certainly have known him, in consultation, declare females to be suffering from stricture, when they presented what I considered to be a mere physiological resistance to the introduction of the sound. Moreover, I have seen many females in whom he had clearly divided the os internum under the same conditions. Indeed, it is well known that the division of the os internum had become a matter of routine with our lamented colleague. I may add that it is so still with many of his followers at home and abroad. Thus, in 1864, Dr. Greenhalgh stated, in my presence, at the Obstetrical Society, that he had divided the os internum in several hundred cases within a few years.

Dr. Marion Sims, on the other hand, in his really valuable work on the *Surgery of Women's Diseases*, seems to think that the os uteri internum can generally be left to take care of itself, and proposes as a remedy for sterility, displacements, and chronic inflammations of the uterus, division of the entire cervical wall on both sides, down to its attachments to the vagina. In midwifery, we are taught to consider laceration of the cervix, during labour, down to its vaginal attachments, a serious complication, and I constantly find it the evident cause and origin of severe chronic inflammatory disease. Yet it is this condition, artificially induced—it is this surgical interference with the natural structure of the uterine neck—that is to remedy sterility, to remove secondary displacements, and to cure chronic inflammatory disease. For myself, I believe that this operation is totally powerless, as a rule, to remedy or even palliate these conditions. Dr. Sims says that he and his colleague, Dr. Emmet, have performed this operation more than five hundred times within a few years in their hospital at New York, and with satisfactory results; but these women should have been seen a year after the operation. No results obtained by uterine treatment of any kind can be accepted as permanent until after that lapse of time.

It must not, however, be thought that I deny congenital or pathological stricture of the uterine orifices. Far from it, I have myself in many cases cured severe congenital dysmenorrhœa or pseudo-membranous dysmenorrhœa by dilatation of the os internum. But I was not guided, in having recourse to such treatment, by the resistance of the os internum to the sound, an unreliable guide, as we have seen. I have resorted to it in congenital cases because there was no other apparent cause for dysmenorrhœa, but evident narrowness of the passages :

in pseudo-membranous dysmenorrhœa, to give a freer exit to the pseudo-membranes; in pathological cases, in chronic inflammatory disease, because the removal of the morbid condition was not followed by spontaneous dilatation of the really narrowed orifice, and by disappearance of the dysmenorrhœa.

As to division of the os externum, I should feel disposed to limit it to those exceptional cases of conoid cervix in nulliparous women, which my friend Dr. Barnes has well described, and in which the orifice of the os internum is preternaturally small. With some women, its rational aggrandisement may certainly facilitate menstruation and conception. But I do not consider it a rational surgical procedure to dilate the cervix, by cutting it in two from apex to base. What surgeon has ever proposed to dilate stricture of the male urethra by dividing the glans or entire penis in two halves?

In conclusion, I would recapitulate by stating that I myself do not consider that there is any positive evidence of abnormal contraction of the os uteri internum if a small wax bougie, warmed and bent to the natural anterior curve of the uterus, passes, on gentle continued pressure, into the uterine cavity. I do not even consider that there is any such evidence on the non-passage of the uterine sound or of the small wax bougie. If there be no dysmenorrhœa, if free painless menstruation exist, the contraction may be vital; it may, and probably does in such cases, relax physiologically at proper times and seasons.

As to the os externum, I see no reason for surgical interference if a good sized wax bougie can be passed easily into the cervical canal. I must confess to being utterly unable to understand how dividing the structures of the cervix, how artificially producing an obstetrical laceration of the organ, can cure these varied diseases. In my opinion the operation, as a routine operation, is a surgical error, and yet it continues to be the routine practice of many eminent obstetricians, and that without the data on which it is founded being given.—*British Medical Journal*, Sep. 21, 1872, p. 321.

## 77.—ERGOT IN ABORTION.

By ALFRED FREER, Esq., Stourbridge.

In a discussion at the late meeting of the British Medical Association, Dr. Evory Kennedy remarked that, "in abortion, ergot was generally inadvisable." As abortion about the tenth or twelfth week is frequently met with in general practice, it is, I think, important that the question of the utility or uselessness of ergot in cases of this kind should be freely ventilated. I have given the drug in above two hundred cases of profuse hemorrhage with threatened abortion, and with the happiest



result. Only yesterday, I attended a woman at the full time with a healthy boy. On February 10th, twenty-five weeks ago, she was taken with great flooding and alarming faintness. She was in the third month of pregnancy, having been "unwell" last in November. I plugged at once with a handkerchief, not being quite able to bring away the ovum, which was protruding to a great extent at the cervix. I then gave doses of the ammoniated tincture of ergot every hour. All bleeding ceased, and the patient went on perfectly well to her full time. I am engaged to attend a lady in October who had precisely the same symptoms in the spring. Indeed, I can call to mind several instances where, ergot being given to check uterine hemorrhage, it has acted well by causing, not the expulsion of the ovum, but its gradual retrocession into the uterus. From what I have seen of the use of the remedy, I am prepared to maintain that it is a most valuable help; but that not unfrequently it does good, not by forcing the contracting uterus to expel its contents, but often, contrary to our expectations, by helping the organ to retain its precious charge to its ultimate preservation. Ergot, in my opinion, is, in threatened abortion, only second to a plug in the vagina; and the best plug I find is a piece of sponge.—*British Med. Journal*, Sept. 21, 1872, p. 327.

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#### 78.—PERINEAL LACERATIONS.

By Dr. JOSEPH G. SWAYNE, Physician-Accoucheur to the  
Bristol General Hospital.

Of all the accidents attending delivery, there is none more common than partial, and none more rare than complete, laceration of the perineum. By partial laceration I mean a rupture extending beyond the fourchette without passing through the sphincter ani muscle; and I do not apply the term to that simple rupture of the fourchette which almost invariably takes place in a first labour. My observations, now, will be limited to these partial lacerations; and to show how common they are, I shall refer to my notes of the last 400 labours which I have myself attended. In all of these the condition of the perineum after delivery was carefully observed. The result is, that there was one case of complete laceration and 31 cases of partial, 25 of which occurred, as might naturally be expected, in primiparæ and six in multiparæ. Now, as there were 101 primiparæ in the 400 cases, it follows that, in about a quarter of these, partial laceration of the perineum took place. Such a result may at first appear startling; but I do not think it would surprise any one who is in the habit of making frequent examinations with the speculum; for it is remarkable how

often one thus discovers the evidences of rather severe perineal laceration. Yet these occurrences, although so common, are very apt to be overlooked. "Out of sight, out of mind" is the general rule in such cases; partly because the practitioner, from regard for the natural feelings of delicacy of his patient, is unwilling to expose the parts to view, and so does not see the laceration either at the time of its occurrence or afterwards; and partly because these accidents cause no very urgent symptoms, and therefore the patient does not direct attention to them. But, although they give rise to no very grave or urgent symptoms, yet they occasion minor inconveniences of different kinds, which it is just as well to prevent if possible. For instance, it is very common to find women complaining of great soreness of the vulva for two or three weeks after delivery, and, on examination, a severe laceration of the perineum is discovered, which still continues in an unhealthy, irritable condition. When a sore of this kind heals after several dressings and applications of caustic, no union of the torn edges takes place, but the perineum still remains in an incomplete condition. The result is, that an uncomfortable feeling of want of support is left; the parts remain too open, and there is a predisposition to prolapse of the uterus and vaginal walls. Although these consequences are not very grave, yet they are of sufficient importance to merit attention and inquiry as to the best means of prevention and cure. With regard to the former, my own experience leads me to endorse all that has been brought forward by various authors, especially by Dr. Graily Hewitt, in proof of the inefficiency of support for the prevention of laceration of the perineum. I have supported the perineum in many cases where rupture appeared imminent, and in many others of a similar character I have left that part without support; and the result from both plans has appeared to be much the same. There is seldom any great danger of rupture, until the head is protruded so far that the accoucheur can grasp the exposed portion with his thumb and fingers. When this can be done, the best plan is to press the head forwards as much as possible under the pubic arch, so as to prevent the *vis a tergo* from acting so directly downwards upon the perineum. When there is any reason to apprehend laceration, it is always as well to have the part in view, so as to be able to give this kind of assistance at the right time; and if the patient be brought close to the edge of the bed, with the thighs well flexed upon the body, this may be managed with so little exposure that she is scarcely conscious of it. Besides, if laceration does occur, it is very difficult to estimate its nature and extent by the touch only, without having recourse to the sight; and that is the reason why many cases of ruptured perineum are



overlooked, until it is too late to obtain union of the wound by the first intention.

When in attendance on primiparæ, I generally, if possible, thus see what is going on, so that I can observe the nature and degree of the rupture just at the time when it takes place, and be able to treat it without loss of time. I have thus seen that, when rupture takes place, the rent usually commences at the fourchette, and extends backwards; but I have almost as frequently noticed it to begin near the centre of the perineum, by two or three small lacerations, and extend forwards. These are generally the least favourable cases for union by the first intention. Almost the only case which I met with, in which the torn edges did not unite when brought together at once, was one of this description. The perineum was, as it were, perforated in two places just in front of the sphincter ani, and from each perforation a rent extended forwards, until the two lacerations met at the fourchette, so as to enclose between them an angular piece of flesh, which subsequently partly sloughed.

With respect to the predisposed causes of laceration of the perineum, one can readily understand why the accident should be common in primiparæ, especially in those in whom, from advancing age, that part has become unduly rigid; but the most frequent cause of all, especially in severe cases, is, according to my experience, an unusual breadth of the perineum. When this is the natural conformation of that part, it will be better, when treating the rupture, not to make any endeavours to produce union of the anterior part of the wound. If the accoucheur does his work too well in this respect, he will very likely have the mortification at a subsequent confinement of witnessing a good deal of it undone by a fresh laceration.

I once met with a curious case in which I found an undue breadth of the perineum in a woman whom I attended in her second confinement. Her first confinement was tolerably easy, and accompanied with very slight laceration of the perineum. Whether, during the healing process after this, adhesion of the lower edges of the labia majora took place, I cannot say; but I found, when examining during the second labour, that the perineum was of most unusual breadth. It for a long time resisted all the natural efforts, and became so tightly spread over the head that it appeared every moment in imminent danger of rupture, whilst the os externum still remained small and undilatable. On examining more carefully, I found a dimple-like depression some little distance from the anterior edge, which appeared to mark the proper situation of the fourchette. I accordingly divided the part in front of this on a director, and the head was born almost immediately without causing any rupture of the remaining portion.

Besides the causes above mentioned, the large size of the child's head and unfavourable presentations, especially those where the head is expelled with the forehead under the pubic arch, will predispose to perineal laceration. In most of the six multiparæ, in whom I noticed perineal laceration, there had been only one previous confinement, and the perineum had then escaped laceration in consequence of the small size of the first child. In the same way an unfavourable presentation in a subsequent labour may cause rupture in a woman who has escaped the accident at several previous ones.

The treatment of partial laceration of the perineum is, I think, most satisfactory, provided it be adopted immediately after the occurrence, or rather I should say, immediately after the termination of the third stage of labour. The best method of treatment is to bring the torn parts together by two or three sutures of fine silver wire. I have generally found that the wound healed more kindly with these than with silk ligatures. An old-fashioned semicircular needle is the best for the purpose. The needle should enter the skin about a quarter of an inch from the edge of the wound, and the sutures should pass through the tissues at about two-thirds of their depth. The operation gives but little pain, as the parts seem to be partially benumbed, and their sensibility impaired by the long continued pressure to which they have been subjected. The number of sutures should be in proportion to the extent of the injury. I have never used more than three; most usually two are sufficient, but in slight cases I have been content with one. As a general rule, when the perineum is not ruptured to half its extent, no treatment will be necessary. I generally remove the ligatures about six days after delivery. Dr. Graily Hewitt, however, removes them at the end of three days. I cannot help thinking that this time is too short, and that by removing them so soon we run a risk of separating the newly formed adhesions. I have never had cause to regret leaving them so long *in situ*; indeed, as I before remarked, cases treated in the way just mentioned terminate most satisfactorily, and I scarcely ever met with one in which union did not take place by the first intention.

I have no time here to enter on the treatment of complete laceration of the perineum. Unless it be attended to immediately after it occurs (which is always the best plan, just as in partial lacerations), the case will be one which will ultimately need the assistance of a surgeon rather than an accoucheur. To show how rare such an occurrence is, I may mention that I never met with more than one instance of it in my own practice, and it happened in the following manner. I was summoned in haste one evening to see a bad case of puerperal convulsions in



consultation with another practitioner. He had previously endeavoured to turn, but had not been able to complete the operation, which was attended with more than usual difficulty owing to the violence of the fits. I succeeded in accomplishing delivery during one of them. After delivery, the fits became less violent, and ultimately ceased, but the patient remained in a state of partial insensibility for some days. Soon after she had fully regained consciousness, she complained to the nurse that she had lost the power of retaining her fæces. On examination, we found that the perineum had been completely ruptured, the rent extending through the external sphincter into the rectum. Owing to the urgent nature of the case, we had not noticed the rupture at the time when it occurred, and I can only account for it by supposing that, from the violence of the convulsive attack just at the moment of delivery, one of the limbs of the child must have been forced through the perineum. The patient was afterwards operated upon very successfully by my colleague Mr. Coe.—*British Med. Journal*, July 27, 1872, p. 93.

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79.—SHOULDER PRESENTATION, WITH RIGID OS AND PROLAPSE OF FUNIS: TURNING IMPOSSIBLE; DELIVERY EFFECTED BY SPONDYLOTOMY.

By Drs. AFFLECK and ANGUS MACDONALD.

Mrs. F., aged twenty-four, residing at Kerr Street, Stockbridge, was taken in labour with her first child, about four o'clock on the morning of the 25th of April, 1872, the first indication of the approaching delivery, according to her statement, being the rupture of the membranes. At this hour the patient experienced one or two feeble pains, which, however, did not recur with regularity or severity till about ten o'clock a.m., when we saw her together. We found that there existed a very long prolapse of the cord, which already protruded from the vagina and pulsated but feebly. The os uteri was very high up, being scarcely accessible to the examining finger, dilated to about the size of a shilling, with thin and hard edges. The cervix was occupied, so far as we could judge, by a rounded mass. It was, however, at this period impossible to ascertain, with anything approaching to certainty, what was the presentation. The woman was, during our visit, almost entirely free from pains. We put her in the knee-and-elbow position, and endeavoured to return the cord, but without success.

Feeling convinced that in this state of matters it was utterly hopeless to attempt to save the life of the child, we desisted after a short time from further interference. During the day the patient had slight irregular pains, which made little im-

pression upon the os. About seven p.m. uterine contractions set in with regularity and with considerable force, and the os uteri began gradually to yield; but long before this time the cord had ceased to pulsate. We saw her together at nine p.m., and came to the conclusion, in accordance with a suspicion which we had already entertained for some hours, but even at this period verified with considerable difficulty, that the presentation was that of the shoulder. The diagnosis was, however, exceedingly difficult to establish, owing to the high position occupied by the presenting part, the rigid state of the os, and the obstruction occasioned by the large mass of prolapsed funis. We also ascertained that there was decided narrowing of the conjugate at the brim, though it was impossible to quantify the degree of contraction. The cervix was now dilated to about the size of half-a-crown, and was, moreover, in a more yielding and less resistant condition. With the view of facilitating the dilatation, we introduced No. 3 of the indiarubber dilators, and distended it to the full, allowing it to remain in the cervix for upwards of a quarter of an hour, the patient having been meanwhile anæsthetized. When the dilator was removed, it was found that the os uteri had expanded nearly so far as to admit the hand. After increasing the dilatation by the use of the fingers for a short period, Dr. Affleck succeeded in introducing his hand within the uterus. He now found that the child lay across the womb, with its back anteriorly, and the head directed to the right of the mother. The introduction of the hand into the uterus was attended with the greatest difficulty, inasmuch as the organ was now in a condition of powerful and persistent contraction, which every attempt to move the hand in its interior intensified. Notwithstanding that the patient was very deeply anæsthetized, the contractions of the uterus were such as to cause Dr. Affleck excruciating pain in the hand which was introduced. After considerable difficulty and some delay, the right foot of the child was reached and brought down into the vagina, into which the left arm of the foetus had by this time prolapsed. Traction was continued with as much force as was considered at all justifiable, whilst efforts were made both to push up the shoulder per vaginam, and to aid version by accessory manipulation over the abdomen. By this time Dr. Affleck's hand was so thoroughly exhausted that Dr. Macdonald relieved him, and continued the efforts at version till, as no success whatever seemed to follow these attempts, it was considered that it would be perilous to the patient further to persist in them. After a little deliberation, it was agreed that the safest and most desirable method to effect delivery was to perform embryotomy in some form or other. After carefully examining the relations



of the parts—and this was facilitated, now that all attempts at version were given over, by pulling down the presenting shoulder by means of traction upon the prolapsed arm—it was found that the spine was sharply bent back over the inlet into the pelvis, and that the head lay so high up in the rigidly contracted uterus, that it was exceedingly difficult, if not altogether impossible, to reach the neck so as to perform decapitation. It was therefore resolved to attempt spondylotomy in preference to evisceration, as the more advisable operation in the circumstances. Dr. Macdonald having procured a pair of long vaginal scissors, which unfortunately happened to have both the blades sharp at the points, proceeded to the operation, whilst Dr. Affleck steadied the body by traction upon the presenting arm, and pressure upon the uterus externally. After a little difficulty, an opening was effected into the thorax, in the left axilla. Into this opening two fingers were introduced so as to push aside the contents of the chest, and feel for the vertebral column; since, owing to the scissors being sharp-pointed, the operator feared to clip round in an intercostal space, as is usually recommended. Having reached the spine at the part where it was most sharply bent, which proved to be the second dorsal vertebra, it was clipped through without any great difficulty. After we had both satisfied ourselves that the spine was completely severed, traction was resumed upon the foot which had originally been brought down, and this time with complete success, although considerable force was needed to bring the breech through the brim. Still greater difficulty was experienced with the head, and very considerable traction efforts had to be employed for a time. The child was, however, removed entire, though the bones of the arm and leg which had been pulled upon, had given way in several places. The foetus was a large well-formed male. The placenta separated spontaneously, and was expelled in about five minutes after the birth of the child. The delivery was completed by two o'clock on the morning of the 26th April. There was no post-partum hemorrhage, and the patient's pulse, when we left, was of good strength, and at the rate of 78 per minute. For the next twenty-four hours she had some tenderness over the uterus, but her pulse kept constantly below 84, and, except on one occasion there was no vomiting. The lochia were copious, and the secretion of milk appeared in abundance on the third day. There never was the least tympanitis, and the patient passed urine voluntarily six hours after her delivery, and continued to do so normally. She has subsequently gone on in every respect favourably, and is now perfectly well.

From our measurements of the pelvis immediately after delivery, the antero-posterior diameter of the brim was not

over 3½ inches, though the patient appears otherwise a well-formed, large, and muscular woman, and is never aware of having suffered from disease of the osseous system of any kind.

Cases of shoulder presentation, complicated with pelvic deformity, are allowed by all accoucheurs to present very serious obstructions to every effort towards effecting delivery, and to form a class about as difficult and dangerous for child, mother, and accoucheur, as is met with in the whole range of practical midwifery. Now, we consider that the present case is one of very considerable interest in many respects, and illustrates very forcibly several important points in the pathology of parturition. It is a known fact that contractions of the brim are much more apt to be associated with mal-presentations than when the inlet to the pelvis is of the ordinary dimensions. The same condition likewise predisposes to prolapsus of the cord, and these two accidents concurred in our case. The premature rupture of the membranes, with the consequent escape of the liquor amnii, increased the complication, if it was not the immediate cause of the prolapse of the funis. Still further, the rigid condition of the cervix, preventing as it did all attempts at turning till the uterus had got into a state of chronic contraction, added seriously to the already great sum of difficulties. Besides, the greatly contracted brim, as it kept the presenting part so high up, rendered the operation which was subsequently had recourse to, as a *dernier ressort*, an exceedingly difficult one.

In regard to the operation of turning, we feel certain that not a moment was lost in having recourse to it, as, so soon as the cervix would admit the hand to pass, the attempt to effect version was begun. Besides, the foot which was grasped, being the right or upper one, proved to be that foot which, had the operator been able to make a selection, he would have preferred, as likely to aid most effectively in rotating the child. But the uterus was so closely contracted and moulded around the child, and the spinal column was so acutely bent in the upper dorsal region, and so closely jammed over the inlet of the pelvis, that no traction upon the foot of the child could in the very least degree induce rotation of the foetus; nay more, from the position of the parts, if it had any effect at all, this must have been to aggravate the malposition by still further depressing the presenting shoulder. We used every legitimate means to elevate the head and shoulder, but all to no purpose, as the rigidly contracted uterine walls resisted all upward movement of the presenting part with perfectly invincible force. We narcotized the patient also to the greatest degree that we felt compatible with safety to her life, but with no beneficial results.



We were, therefore, ultimately driven to one or other of the various forms of embryotomy, and we think the one adopted was the most judicious, and, as it proved, both a quick and safe one. In the position of the foetus with the head so high up, and aggravated by the narrowness of the inlet of the pelvis, decapitation was impracticable. The blunt hook was attempted to be put round the neck of the child, with the view of testing whether the neck might not be brought within operative reach, but this proved impossible. To wait for the chance of a spontaneous version in such circumstances, with such markedly contracted pelvis, or for any chance of relaxation of uterine fibres, seemed to us equally hopeless and dangerous. The operation of evisceration is well known to be both tedious, troublesome, and difficult; while it presents facilities for ultimate delivery inferior to the operation of spondylotomy, and is certainly more dangerous for the patient. Spondylotomy is, however, by no means a very easy operation to perform, more especially when executed with inferior instruments, as on the present occasion. The difficulty in our case consisted greatly in the risk that was incurred of passing the point of the scissors through the body of the child into the parts of the mother. This risk is, however, perfectly easily eliminated by providing one's self with scissors, one of the blades of which is probe-pointed. In the present instance, all risk to the mother was as far as possible prevented by the scissors being completely guarded in its introduction by the fingers and hand of the operator, and by the separation from the primary incision into the wall of thorax, on to the spinal column, being made by dilating with the fingers, without the employment of the scissors. An additional difficulty had, however, to be met, by the position of the arm being such as to place the spine of the child at the point of its body furthest away from the presenting part. The division of the spine, however, being at last effected, it was surprising with what facility the uterus allowed the breech to be brought down, and the extraction of the body to be effected. The explanation of this facility is, however, not difficult to find. In such cases the uterus has become so rigidly contracted and moulded round the foetus, that, with the normal length of the latter conserved, it is impossible to effect version. But the incision made through the spinal column breaks the resistant bar which the child's trunk forms into two unequal portions, either of which has plenty of room to be moved with ease in the uterus, contracted though it may be. We have thus all the mechanical advantage from this operation which we could derive from cutting the child completely in twain, whilst the small opening made into the body of the foetus allows no projecting portions of bone to injure the uterus in the expulsion

of the child. Had it not been that the case presented the additional difficulty of contracted pelvis, it might have been left alone, after section of the spinal column, to be finished by spontaneous expulsion. But the contraction rendered this termination highly improbable, and as there was comparatively little force needed in bringing down the breast to the inlet of the pelvis, such delay seemed to be unnecessary.

In looking into the literature of this operation, it seems to us not to have got that attention which its very great advantages ought to secure for it. As far as we have been able to trace its history, it seems to have been first recommended by Michaelis of Kiel, who is referred to by Schroeder as having originated it. But we have not been able to verify the reference, in consequence of the impossibility of getting hold of the periodical referred to (*Neue Zeitschrift für Geburtshülfe*, Band 6, s. 50). In a case recorded by Prof. Inglis, of Aberdeen, in the *Edinburgh Medical Journal* for February, 1866, Sir James Y. Simpson performed it successfully. According to Dr. J. W. Black, as shown in a footnote appended to the reference to that case, in *Simpson's Selected Obstetrical Works*, page 502, Sir James Y. Simpson had previously effected speedy delivery by cutting through the spine without eviscerating, in two cases, so that Dr. Inglis's case seems to have been the third in which Sir James had performed the operation. Undoubtedly we owe to Sir James Y. Simpson the introduction into obstetrical literature of the very expressive and definite term for the operation, spondylotomy (from *σπόνδυλος*, a vertebra, and *τέμνω*, I cut). As far back at least as 1854, Drs. Priestley and Storrar, in their edition of his *Selected Obstetrical Works*, mention that Sir James was in the habit of recommending section of the spinal column in cases of cross-birth, in which turning was found impossible. So far as we can make out, however, Michaelis wrote before this period. Whether Sir James, who does not seem to have laid any claim to the operation, was aware of his writings on the subject or not, it is beyond the range of our knowledge to determine. In his *Principles and Practice of Midwifery*, Dr. Milne gives a concise and clear account of the method of operating, and refers to one or two cases which were reported in the *Lancet* of last year.

One of these cases is recorded by Mr. Orehard, of Kingussie, and was performed as early as 1847. His report in the *Lancet* is as follows:—"Mrs. R., about thirty years of age, was in labour with her fourth child. She had a contracted pelvis, and I had on two occasions delivered her by forceps and on one by embryotomy. On this occasion the arm presented, the water was evacuated, and she had been many hours in labour before I saw her. I made several attempts to turn, but it was impossible,



the child lying across the uterus, rigid and immovable. I could lay hold of the inferior extremities, but no force could move it. Seeing no prospect of delivering by turning, and the patient becoming a little exhausted, I determined to deliver by cutting instruments. I used a pair of strong scissors. I introduced the point of the scissors through one of the intercostal spaces, and clipped along the space to the spine, and with the scissors cut through the spine. I then introduced my hand, seized hold of the feet, turned the child with the greatest ease, and delivered the patient. She made a good recovery. The plan I adopted is, I think, much superior to the ordinary one of disembowelling the child, and extracting it double, or tearing it out piccemeal. When the spine was cut it seemed to act like a hinge."

In the *Lancet* for 18th February, 1871, Dr. Tucker, of Bermuda, states that he performed this operation on the wife of a private in the 61st Regiment; but that he first eviscerated the thorax and abdomen, and then crushed the spine with a pair of tooth-forceps for want of better instruments. This he had performed successfully in a previous case. As Dr. Tucker first eviscerated, then cut through the spine, and finally effected delivery by bringing the foetus out doubled up, his method is only, after all, a modified form of embryulcia.

That both in regard to rapidity of execution, and safety to the mother, spondylotomy is very far superior either to decapitation or to disembowelling, there seems to us to be no doubt whatever. To decapitate is, under ordinary circumstances, no easy task, and in the one before us would have been extremely difficult, nay, we think almost impossible, owing to the contraction of the pelvis, and the high position of the head in the rigidly contracted uterus, while it is an operation fraught with no little danger as ordinarily performed, since it is exceedingly difficult to prevent the sharp hook of Ramsbotham or any similar instrument from penetrating the mother's parts. Also the projecting bones in the ends of the two portions into which the foetus is divided present an additional difficulty from their liability to injure the uterus. Besides, in a case like ours with contracted pelvis, we would, in a measure, merely have been exchanging one difficulty for another nearly as formidable, if we had divided the neck; since, while the body would have been removed with no great difficulty, the head would have offered great resistance to every endeavour to pull it through the contracted brim. Most likely we should have had to diminish its size, and get at the same time a firm crotchet hold upon it by seizing it with Simpson's cephalotribe, which we had brought along with the scissors in case of emergency. Besides this, cutting through the whole of the neck with scissors instead of with Ramsbotham's hook, even when it is applicable, as we

hold in this case it was not, is both a much more difficult and dangerous operation than spondylotomy; seeing that, in performing the latter operation, the mother's parts are completely protected by the screen formed of the child's parts, and thus risk of wounding the uterus is greatly avoided.

One method of delivering in these cases, of recent introduction, possesses such a degree of simplicity, however, as to merit special notice when we are on the subject.

In the *Archives Générales de Médecine*, September 1865, Professor Pajot contributes a paper on the presentation of the shoulder in extreme contractions of the pelvis, and on a new method of embryotomy, which latter consists in getting a piece of whip-cord round the neck or body of the child, and performing section by means of it. One end of the cord is to be passed over the neck or body of the child by means of a blunt crotchet in which is a groove for the string. A piece of lead is attached to the end of the string, which is passed over the child so as to facilitate its downward movement over the foetus. The two ends of the cord are then to be secured in a tube or speculum formed of wood, introduced into the vagina so as to prevent the maternal parts being injured by the tensified cords. The ends of the cord he then recommends to be rolled in succession separately round each hand of the operator until about nine inches from the vulva; then laying hold of each end of the thread by the base of it, one after the other, he recommends the execution of a rapid series of to-and-fro motions in a sawing manner. Thereby, he states, that the neck of the foetus can be separated from the trunk in some seconds. In the case where the neck is inaccessible, he states that section of the body is quite easily performed in the same manner, only that to divide the trunk in the region intermediate between the iliac crests and the scapulæ requires in general from four to five minutes. The great advantage of this method is, of course, its simplicity, and the very trifling number of instruments that are absolutely required. In this latter case it is almost superior in simplicity to spondylotomy, which needs tolerably strong scissors. It is scarcely possible to believe, however, that whip-cord could have sufficient resistance to make section of the neck or trunk of the child; and, of course, this operation, even when successful, does not do away with the risks of laceration of the mother's parts by means of projecting fragments of bones; whilst in the case of decapitation, it lands us with all the difficulties of removing the head last from the uterus. So convinced is Pajot of the risk attending the ordinary method of delivery in cross births in cases of contracted pelvis, that he recommends—1st, In cases of contraction under two-and-a-half inches, when the infant is at full term, and



version by external manipulation has been fairly tried with the view of facilitating the application of instruments, and has been considered impossible, the performance of cæsarian section; 2nd, When the foetus is not at the full term, and version considered impossible, to favour spontaneous evolution by amputation of the arm; 3rd, If this does not succeed, to effect embryotomy according to his own method.—*Edinburgh Medical Journal*, July 1872, p. 39.

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#### 80.—A SUCCESSFUL CASE OF IMMEDIATE TRANSFUSION.

By Dr. J. H. AVELING, Physician to the Chelsea Hospital for Women.

The following satisfactory case of transfusion proves that the old "immediate" method of performing the operation has many advantages—so many, indeed, as to lead to the belief that it must ere long be adopted by the profession. For one hundred and fifty years no other plan was known; and it is only during the last half-century that "mediate" transfusion has been in vogue. The delay and difficulty in operating and the deteriorated condition of the blood transmitted are the great objections to this latter method. If it can be shown that the immediate plan is as successful as it is simple, transfusion will assume its proper place among the remedies of the healing art, and will be used both by physician and surgeon in all cases where deterioration or loss of blood threatens extinction of life.

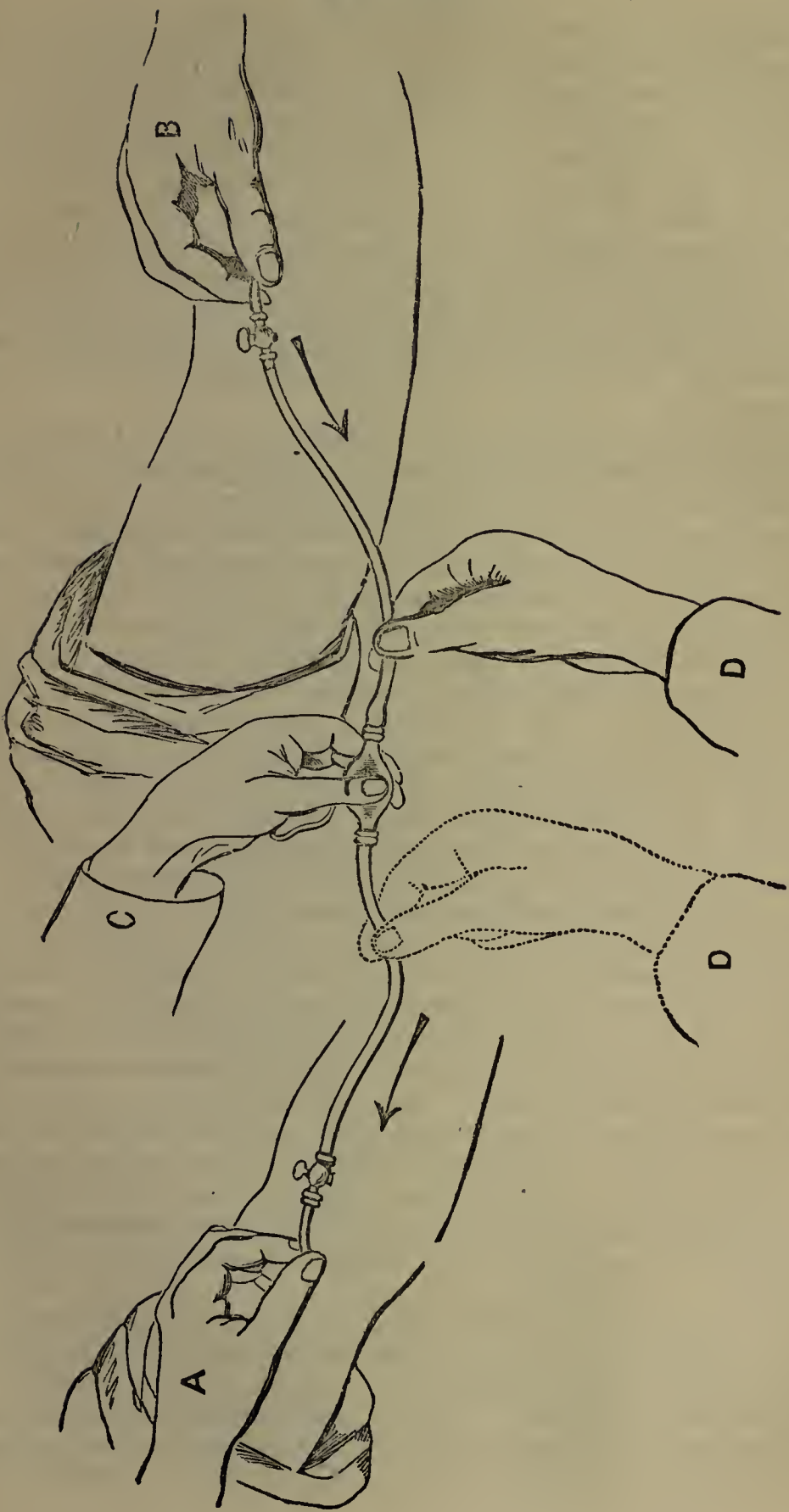
Mr. F. E. Webb, of Maida-vale, has kindly furnished the following notes of this case previous to the time of my being called in:—

"Mrs. W., a small, fair lady, aged twenty-one, of rather delicate constitution, was seized with abdominal pains on the evening of March 24th. Early on the morning of the 25th I was sent for. The pains were frequent, but there were no expulsive efforts. The os was slightly dilated and the vaginal secretion abundant. Labour proceeded slowly, owing to the brim of the pelvis being narrow; and the child's head did not descend into the cavity of the pelvis until one o'clock, when sharp expulsive efforts commenced. At two o'clock I began to give her chloroform at intervals, but not to insensibility at any time, until the child was born at 3.30 p.m. As soon as the child was detached smart hemorrhage set in. I sent for ice, gave ergot and brandy freely, and, grasping the uterus, excited it to vigorous action. The hemorrhage continuing, I found it necessary to detach the remaining half of the placenta, which was unusually adherent to what felt like the partially inverted

fundus of the uterus. This I endeavoured to replace, but without success, as I was obliged to desist my efforts at the time lest the shock should entirely extinguish life. Blood continued to flow freely, and the patient became rapidly exhausted and faint, and no pulse could be felt at the wrist. Ice in the vagina at length checked the bleeding; but, as there seemed every probability of the patient dying, the husband was requested to go for further advice. Dr. Cheadle came first, and shortly after Dr. Meadows. At this time (4.30 p.m.) there was no great amount of hemorrhage, but we all agreed that the only prospect of saving the patient's life was to transfuse some blood into her veins. Dr. Cheadle kindly went for Dr. Aveling, knowing that he would probably have the necessary apparatus, and would be ready to perform the operation."

I found the patient in a most dangerous state of exhaustion, insensible, and no pulse to be found in either the temporal or radial arteries. The pupils were dilated, and did not contract when a light was placed near them. The hands and feet were cold, and the lips and faced blanched. The heart's action was weak, and growing perceptibly more feeble. It was evident there was no time to be lost. A fold of skin at the bend of the patient's arm was raised, transfixed, and divided; when a large flattened blue vein became visible. This was opened, and the afferent tube with some difficulty, on account of the insufficient light of two candles, was adjusted. The arm of a coachman in the employ of the family was next prepared as in ordinary bleeding, and an incision made directly into the vein sufficiently large to admit the efferent tube. The man was then seated in a chair beside the bed, and the india-rubber portion of the apparatus filled with water having been attached to the tubes the process of transfusing commenced. After a few drachms had been transmitted, Dr. Meadows, who kindly took charge of the afferent tube, thought he felt the skin rising near the incision, and suggested that the tube was not in the vein, but in the cellular tissue beside it. This proved to be true, and the tube had to be taken out and inserted into the vein. Its collapsed condition and the want of light made this no easy task; but it was at length effected, and the transfusion then went on steadily and easily until more than sixty drachms of blood had been injected. As the operation proceeded, the pulse at the wrist became perceptible, the lips less blanched, and warmth returned to the hands. The patient also became conscious for a short time, and said she was "dying." The mental improvement was not as marked and rapid as I anticipated; but this was, perhaps, due to the quantity of brandy she had taken. In a few hours, however, she became quite conscious, spoke, took nourishment, and began her fresh lease of life.





The wound in her arm healed by first intention ; but it opened again a few days after to allow some pus to escape, the result of the accident already alluded to. When the patient was sufficiently recovered she was placed under the influence of chloroform, and the uterus, which had become completely inverted, was returned to its normal position. After this operation Mrs. W. improved rapidly, and is now quite well. It would be ungrateful not to admit that a large part of the success of the operation is due to the able assistance I received from Dr. Meadows and Mr. Webb, and I must add, from the coachman, who was not only collected and cheerful, but able to make several useful suggestions during the process of transfusion.

*Mode of operating.*—The patient having been brought to the side of the bed and the arm bared, a fold of skin over a vein at the bend of the arm is raised, transfixed, and divided. The flattened vein now brought into view should be seized with a pair of fine forceps, and raised while an incision is made, and the bevel-pointed silver tube inserted into it. The tube should be filled with water, and kept full by placing the thumb over its larger opening. While the operator is doing this an assistant should prepare the arm of the blood-donor as in ordinary bleeding, making an incision direct into the vein, and passing the round-pointed tube into it, with its point towards the fingers. This person should then be brought to the bedside of the patient, and seated in a chair. No ligatures are required. B represents the hand of an assistant holding the efferent tube, and the lips of the small wound together, and A shows the afferent tube secured in the same manner. The india-rubber portion of the apparatus, filled with water and kept so by turning the cock at each end, is now fitted into the nozzles. The cocks are then turned straight, and the operation commenced by compressing the india-rubber on the efferent side, D, and squeezing the bulb C. This forces two drachms of water into the afferent vein. Next shift the hand D to D', and compress the tube on the afferent side ; and then allow the bulb to expand slowly, when the blood will be drawn into it from the efferent vein. By repeating this process any quantity of blood can, at any rate, be transmitted, the amount being measured by counting the number of times the bulb is emptied.

This instrument was invented and shown to the Obstetrical Society in 1864. Dr. Oré, who published an excellent work on transfusion in 1868, describes an apparatus similar to mine, except that it has valves. These are objectionable, inasmuch as they get out of order, and whip the blood when passing through them. He agrees with me, however, concerning the



relative values of the "immediate" and "mediate" methods. He says, in performing transfusion by the immediate plan, his experience has shown that embolism is easily avoided, and that the defibrination of the blood is useless in man, as it does not coagulate for four or five minutes after it has left the vessels.—*Lancet*, August 3, 1872, p. 147.

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# 81.—EPILEPTIC PUERPERAL CONVULSIONS AND PUERPERAL MANIA TREATED BY CHLORAL HYDRATE.

By F. VICTOR M'DOWELL, Esq., Ex-Demonstrator of Anatomy,  
Dublin School of Medicine, Ballickmoyler, Queen's  
County.

On the 1st of March, 1871, at 12 p.m., I was called to see a poor woman in labour, named Ellen B., aged 40, living in this district. I was told she had been in "fits" all the day. She is the mother of seven children; the last was still-born, and her health has been very delicate since. For the past month she complained of giddiness and noises in the head, and puffing of the face. On my arrival I found her suffering from epileptic convulsions. The paroxysms occurred periodically, like labour pains, during one of which, and shortly after I had seen her, she gave birth to a still-born child. The placenta was soon expelled without any assistance, and with very little loss.

The patient was totally unconscious, and breathing very heavily; however, the convulsions did not end here, but after a few hours became more frequent.

At the approach of each paroxysm the pulse would become extremely quick and weak, the pupils dilate, the skin turn dark or purple, and the muscles become extremely rigid from head to foot, but would soon be thrown into violent convulsions, during which the distortion of countenance was beyond conception. There was frothing and hissing from the mouth, which was drawn chiefly to the right side, her urine and fæces passing involuntarily. Her head was shaved with much difficulty, four leeches applied to each temple, an opiate enemata administered, and cloths soaked in iced water to be kept constantly on the head.

The "fits" which, previous to this treatment, occurred every hour, and each lasting about ten minutes, were completely checked, and the woman left sleeping tranquilly.

Heard next morning she awoke *quite well*, but after three or four hours became light-headed.

Saw her next day, and she was then totally unmanageable; the delirium and jactitations were incessant, restraint being sometimes necessary. The patient was now placed in a

darkened room, with a careful attendant, the cold application to be kept to her head, and a large blister applied to the nape of the neck. A full dose of calomel and jalap administered, and after its operation two grains of James' powder and half a grain of opium, to be given every three hours for six doses. For five days there was no abatement of the maniacal symptoms, and as the patient had not slept any during that time I determined, as a *dernier resort*, to give chloral hydrate a trial, as I had never heard of its use in such a case before. Twenty-five grains were administered in syrup, and repeated after three hours. This seemed to act as a charm, for the woman had a refreshing sleep of three hours; and a repetition of this treatment for a few nights effected a complete recovery, as a proof of which I may mention I received a visiting ticket on the 5th of March this year, to see her again in labour, and as I found the os well dilated I applied a long forceps. and delivered her of a fine living child, and both are doing well.

It will be seen the above was a remarkable case, and those experienced in dispensary practice can well realize the difficulties of treating such—the convulsions occurring during and after parturition, and terminating in mania.

There was no anasarca. I regret failing to test the urine, and have no further proof of albuminuria than the puffing under the eyelids. The previous history of this patient failed to show that she had ever been attacked by epilepsy.

Much difference of opinion exists as to the action of opium in puerperal convulsions; however, I found it act most favourably in this case, and would certainly give it a trial again, provided there was no tendency to apoplexy.

It proved useless in the maniacal affection.—*Dublin Journal of Medical Science*, June 1872, p. 464.

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## 82.—THE TREATMENT OF INERTIA OF THE UTERUS.

By LAWSON TAIT, Esq., Surgeon to the Birmingham Lying-in Charity and to the Hospital for Women.

In August last, I was summoned hurriedly one morning to see a woman in labour and attended by a midwife. I found her cold, pulseless, and the bed and floor swimming with blood. It scarce needed an examination to know that the placenta was presenting. The os readily admitting my hand, and the uterus being perfectly flaccid, I at once removed the placenta, and, by the combined version, easily brought down both feet. I could get no amount of manipulation to induce contraction of the uterus; and to wait for the action of ergot was to waste time. I therefore passed my left arm under the patient, and



my right arm over her, embraced the uterus between them in a direction parallel with the axis of the brim, interlocking my fingers and spreading them over as large a part of the fundus as possible. In this position, and by the external pressure, I imitated normal contractions, as one does with the forceps, and soon had the satisfaction of finding the child extruded as far as the shoulders; then, with the left hand on the fundus, and the right guiding the child, birth was soon completed. The child was almost dead, of course; but the mother made a good recovery.

Since this case, I have had on three occasions to adopt the method of external pressure in cases of inertia.

Mrs. K., confined on December 20th of her seventh child, had a large roomy pelvis; but though the passages were relaxed to the fullest and ergot administered twice, the uterus was absolutely passive. I placed her at once under chloroform, and commenced the intermittent pressure described above. In about ten minutes delivery was completed, and there was no *post partum* hemorrhage.

In September, I was retained to attend Mrs. E., the mother of seven children. Her attendant informed me that nearly all her labours had to be assisted by the forceps on account of an inert state of the uterus, and that in the last two she had nearly lost her life from *post partum* hemorrhage. A few days before her confinement, the house was entered during the night by burglars, and she awoke to see one of them removing her husband's watch from the toilet-table. She was, of course, much excited, and in the morning I was asked to see her. We feared very much that the fright would exercise an unfortunate influence, and so, indeed, it proved, for symptoms of labour came on immediately. We tried by opium and rest to delay it, and succeeded for a time. At last, however, it came on, and, as before, the second stage was characterised by inertia. The pains were sufficiently severe to require anæsthesia, but whether it was continued or not, they did nothing. Instead of resorting to the forceps, as I originally intended, I employed the external pressure, and very soon had the head on the perineum. I then used the forceps to guide the head through the passages, still having the external pressure kept up. She recovered more rapidly and more completely than she had ever done, according to her own account, though she attributed this to the chloroform. It is certain that she had no after-hemorrhage.

The third case was in the person of a strongly built woman, in whose second labour the pains suddenly ceased without apparent cause. The head was nearly on the perineum when I arrived to assist the midwife. I advised the external pressure instead of ergot, and we found it perfectly successful in re-

establishing the pains, and by its help labour was completed.

I feel convinced from even this short experience, like Dr. Playfair, that the method of external pressure will soon and in most cases supersede all uterine stimulants, and that the use of these will now be confined to the *post partum* state. External pressure is of great help when, from accident or necessity, anæsthesia has been pushed so as to interfere with the intensity of uterine action. I have little doubt that this communication will bring many others from obstetricians who have practised the same or some similar method of external pressure. My plea is that, so far as I know, it is not used in this country as it ought to be, and that ergot is far too largely consumed by parturient women, especially when under the care of the so-called "trained midwives."

[Since correcting the proof of this note, I have seen a reference to a method of assisting the inert uterus by external pressure in Dr. Meadows' *Manual*, with allusions to the writings of Ritzen, Kristeller, and Playfair, but I have as yet had no opportunity of going further into the history of this method of treatment. It would seem to be not of older date than 1856.]  
—*British Medical Journal*, June 22, 1872, p. 668.

### 83.—ON DYSMENORRHŒA.

By DR. J. MATTHEWS DUNCAN, Edinburgh.

[This intractable affection has been variously subdivided by writers on the subject. It is indeed merely a symptom, and the subdivisions indicate what are believed to be the primary diseases causing it. The present paper treats chiefly of]

*Spasmodic Dysmenorrhœa*.—This is the chief form of the disease. When present in a high degree it is intense suffering. The pain is often so great as to produce writhing, sometimes sickness and vomiting, sometimes prostration with cold sweating. The pain is described by the sufferer as spasmodic; generally remitting, sometimes nearly completely intermitting. It may occur at any time during menstruation, or even while menstruation is absent. Generally it is severest in the early days of the discharge, and frequently most so on the second day, or before the menstrual blood flows freely.

The disease, as it comes before the physician, is generally chronic. It may have begun insidiously, and gradually increased. When it has once begun it returns with great regularity at the periods, but its intensity at different periods is liable to great variations. It is common among the unmarried, specially affects the sterile, commencing after marriage, and much less frequently the fertile.



I have not been able to arrive at any satisfactory results as to the etiology of the disease. Only it is sure that sterile marriage is frequently in some way or other a cause of it.

This disease has been long regarded as being produced by mechanical obstruction to the passage of the menstrual fluid out of the cavity of the womb, and latterly, the prevalence of this view has greatly increased, and the term stricture has been applied to the obstructing cause. The pain is then described as being produced by uterine efforts to expel the retained menstrual fluid; but real bearing-down expulsive efforts are an uncommon occurrence in this disease. There are great difficulties in the way of realizing a satisfactory theory of spasmodic dysmenorrhœa. Yet this common mechanical theory is untenable in the present state of our knowledge. The disease which naturally occurs to the mind as the analogue of so-called mechanical dysmenorrhœa is stricture of the urethra. In both, there is believed to be partial closure of the natural passage for the discharge of an accumulated and accumulating fluid. In both, mechanical means of cure are recommended, and with one merely mechanical object in view—enlargement of the passage.

The more true analogy for spasmodic dysmenorrhœa appears to be after-pains. In both diseases there is pain, uterine and of a spasmodic kind, while a bloody fluid is exuding from the interior of the uterus and getting free discharge through the cervical uterine canal. Both diseases are generally most urgent during the first and second days, and then diminish, while the discharge continues to flow still for some days longer.

The following difficulties in the way of acceptance of the mechanical or stricture theory may be stated.

The stricture or mechanical obstruction is not demonstrated. The disease is a common one; yet, on examining the uterus, its canal is, with rare exceptions, found to be as patent as in healthy women, or as in women who have no dysmenorrhœa.

The cutting instruments and tents used in dividing or dilating the supposed obstructing part or stricture are so large as to be available only when there is no stricture. Their use is itself proof that the mechanical theory is erroneous, or that mechanical obstruction does not exist.

When, in rare exceptional cases, a stricture is found—generally a congenital stricture of the external os—dysmenorrhœa is not always observed to coexist with it. In other words, extreme stricture may exist without dysmenorrhœa.

When, in rare exceptional cases, a real narrowing is found, as in congenital stricture of the external os, accompanied by dysmenorrhœa, then cure of the stricture is not found to be reliable as a cure of the dysmenorrhœa.

When there is imperforate hymen or complete atresia of some part of the neck of the womb, and consequent dilatation of the uterine cavity by retained menstrual fluid—that is, when there is absolute obstruction to the discharge, not merely a stricture—then the pains complained of are not so severe in kind or degree as in a characteristic case of spasmodic dysmenorrhœa.

Not only is there not described any stricture or mechanical obstruction, such being only imagined; but the dilatation and hypertrophy of the uterine cavity, which should be the natural result of such mechanical obstruction, are not described, and not found.

Characteristic dysmenorrhœa is observed at menstrual periods which are scanty in flow. Indeed, many authors describe scantiness of flow as the ordinary condition of the disease.

It is often observed that when the menstrual flow becomes copious the pain ceases, or that when it is copious from the beginning, the pain is slight or absent—circumstances quite inconsistent with the stricture theory.

Violent dysmenorrhœa is often present (if we may be pardoned the apparent contradiction in terms) when there is no flow, and consequently when there can be no obstruction, as in cases of imperfectly developed uterus, or cases where the uterus is replaced by a fleshy tubercle.

The disease sometimes disappears unexpectedly, and, after some healthy periods, returns again; a circumstance quite inconsistent with the existence of a permanent contraction or stricture.

The stricture theory fails at every point; for even were there a considerable stricture, yet not an absolute closing of the canal, it is impossible to regard it as an obstruction to free discharge. A very small opening—that is, much smaller than that of the natural or ordinary uterine size—is sufficient to allow easy passage to a much greater quantity of blood than seeks discharge. It is not true that any described size of passage (including the quality of length) in cases of stricture will impede the passage of any ordinary menstrual flow. This may be, and indeed has been, demonstrated by experiment. It is to the destruction or removal of the stricture that treatment is directed by the believers in the mechanical or obstruction theory. The very frequently unsatisfactory results of that treatment do not justify much confidence in the evidence supposed to be afforded by it in favour of the theory. Indeed, except in the case of the external os uteri, it has not been shown that the result of the mechanical treatment is permanent enlargement of the passage or removal of the stricture; and it is not natural to expect that such a result should follow. Incisions, at least, in the length of a mucous canal will not enlarge it, but rather have the opposite tendency.



Supporters of the mechanical theory must meet the case against them as here stated, for the most frequently occurring conditions have been supposed. In addition, they have to show what appears to me to be intensely improbable, that the force of the movement of the blood as it is discharged from the mucous membrane is so great as to distend the cavity of the body of the dense unimpregnated uterus actively and quickly.

But the theory may perhaps be applicable in special circumstances, which I shall now consider.

The canal may be closed by a plug of blood or dense mucus. This, indeed, would better account for the symptoms supposed to arise from obstruction than the imaginary stricture. But there are great difficulties in accepting this theory. For, although clots are sometimes seen in dysmenorrhœal discharge, they are by no means a necessary or even an ordinary occurrence. Again, clots would in any uterus act as injuriously as in one that is the subject of stricture, and they are not rarely observed in menstruation without there being any dysmenorrhœa. Further, if clot-plugs were the obstruction, they would stop the discharge entirely till they were displaced—a stoppage which is not described in this disease. Besides, several of the arguments against the so-called stricture tell against this supposed form of obstruction.

In like manner, many of the above detailed arguments may be made to tell against the supposition that the partial or complete closure of the cervical canal by spasm may be the obstructing cause.

It has been said that the obstruction is at the external os uteri. But this notion is entirely without foundation of any kind. Curiously, and also ridiculously enough, this allegation is made by gynæcologists who recommend incisions for the removal of the imaginary obstruction to be extended as high as the internal os uteri.

This latter part is with some reason fixed upon as the site of obstruction. It is the narrowest point of the whole uterine passage, and it is much less dilatable than the external os uteri. Besides, other indications might be adduced. But, as I have already said, no sufficient stricture has been shown in dysmenorrhœal cases to exist here; indeed, none at all. Not only does no morbid stricture exist in these cases, but the history reveals no cause of such disease coming on. Sterile marriage, for example, is a frequent alleged cause; yet it would be a bold and unjustified assertion that sterile marriage is a cause of stricture. Surgeons have not, as yet at least, found it to be a cause of urethral stricture in the male.

The analogues of dysmenorrhœa of the kind under discussion appear to me to be after-pains, strangury, tenesmus. These

two last affections do, indeed, sometimes exhibit their affinity with it by accompanying it.

Patients frequently distinguish between neuralgic and inflammatory dysmenorrhœal pains, and those of a spasmodic kind. Not rarely those of one kind disappear while those of another kind remain.

When the uterus of a sufferer from dysmenorrhœa is examined during the absence of the flow, there may be nothing amiss discovered. It is a great rarity to discover anything of the nature of a stricture, for this is a rare malformation; and I can only here express my surprise at the language used by physicians, in describing to patients the nature of their cases. Such expressions as "the womb closed," "the passage too narrow," and "stricture," are frequently and certainly inappropriately used. But frequently the following conditions are observed:—An ordinary full-sized uterine probe passed into the uterus finds great and unusual sensitiveness or tenderness at the internal os of the cervix, and in the cavity of the body of the uterus; sometimes the sensitiveness is extreme, and the pain excited very violent. If the probe be allowed to remain, and the patient is asked what is the nature or character of the pain, she very frequently spontaneously describes it as the pain she has during her monthly period. A large probe, a No. 10 or 11 of the ordinary male bougie series, can frequently in the intensest cases of spasmodic dysmenorrhœa be easily introduced, if a little pressure is kept up for a minute or so, on the obstructing internal os uteri. Frequently, indeed; a much larger bougie can be passed than a 10 or 11. Sometimes, in so examining a case of this kind, temporary and partial spasmodic obstruction at the internal os can be made out. There is no difficulty or obstruction usually at the external os uteri. When a large bougie is passed through the cervical passage to the fundus, and allowed to remain, the spasmodic pain excited, at first very severe, gradually diminishes, and is almost, if not entirely, gone in less than a quarter of an hour. The dilatation of the internal os is then so far effected, and probably the irritated fibres have relaxed their spasms. The pain is again momentarily re-excited in withdrawing the bougie. The pain thus produced, is the pain of the spasmodic dysmenorrhœa, but generally severer, as might be expected. It appears probable that the pain of dysmenorrhœa is produced by the flow of blood from a uterus whose irritability is increased by the performance of the function of ovulation, and by its concomitant phenomena.

When a tangle-tent is introduced through the internal os uteri, and allowed to remain, the history of the pain produced is generally as when a metallic bougie is introduced, only it lasts longer. When a metallic bougie is introduced, the dilatation



is completed at once, for the time at least; but when a tangle-tent is introduced, this is not true, because, after the dilatation caused by its introduction, further dilatation is caused by the expansion of the tissue of the tent. Yet even with a tangle-tent, the spasmodic pain generally is at an end in some hours. Not always so, however, and this generally in characteristic or bad spasmodic cases. In them, twenty-four hours may elapse, and the patient be still writhing under the incomplete or still-proceeding dilatation. When, in these circumstances, an attempt is made to withdraw the tent, it is found difficult to effect the purpose; for while the parts of it above and below the internal os uteri are expanded, the part encircled by the internal os has not had as yet sufficient force or sufficient time to effect the dilatation of that part. The tent is thus firmly seized by the stricture. This stricture is found to be under half an inch from the point or inner end of the tent, and it is more abrupt at the lower than at the upper or inner side. If the tent is allowed to remain longer, it at last effects dilatation to its own full expanded size. It wears out the spasm, probably.

By using successively larger and larger bougies, and allowing due time for the relaxation of spasm produced by each successive increase in size of bougie, it is easy to dilate the whole uterine passage largely, that is, till an 18 or even a larger bougie is passed.

In studying the theory of spasmodic dysmenorrhœa, it is worth while to remember the small size of the uterine extremities of the Fallopian tubes, and the possibility of spasm in these tubes, and of difficulty in the progress of the ovulum or other discharges from the tube into the uterus.

The diagnosis of dysmenorrhœa is easily made. It is painful menstruation, and it has only to be ascertained that the patient is probably or really menstruating. To diagnose simple dysmenorrhœa, it is necessary to ascertain the absence of any complicating uterine disease which may induce the pain directly or indirectly, as a uterine fibroid.

To diagnose the kind of dysmenorrhœa is generally easy, so far as such diagnosis goes. The different kinds may be conjoined; a dysmenorrhœa may be at once neuralgic, inflammatory, or congestive and spasmodic. To describe the diagnosis would be merely to repeat what has already been said under the head of symptoms.

The treatment of spasmodic dysmenorrhœa is highly unsatisfactory.

In slight cases, relief is gained by the popular remedies directed to increase the discharge. These are the hot pediluvium, and the swallowing of alcoholic stimulants, or of sal volatile. In somewhat severer cases, benefit is got by the use

of antispasmodics, and the weaker sedatives. Among such remedies, the chief are ether, hyoscyamus, Indian hemp. Diaphoretics are also useful.

In the severer cases, all such remedies are of no avail. Relief can be secured by anæsthetic inhalations, or by the use of opium in some form. Many authors recommend the use of special preparations of these kinds of medicines, and often special methods of using opium, as by the mouth, by enema, by hypodermic injection: but after many trials of all methods, I have failed to discover any decided advantage in one method or another.

It is obvious that this kind of treatment is not in any sense curing. It is a poor compromise which really consists in the drowning of a pain, or a painful disease, by another artificially-produced disease, which is probably more injurious to the constitution generally. Intoxication for a day or thereabouts by chloroform or by opium every month, is a morbid condition which has many disadvantages and dangers of its own. Yet there are cases in which the pain of dysmenorrhœa is so severe that the practitioner is glad to resort to these drugs.

Dysmenorrhœa is often associated with sterility. Both are conditions which induce women to insist upon treatment, and to make them regardless of risk. But it requires to be kept in mind by the practitioner, that he is not justified in using a remedy whose proper risk is out of due proportion to that of the disease which he is treating. A so-called radical cure has long been in use for this disease. It is dilatation of the cervix uteri, and its introduction was a matter of course, when the mechanical or stricture theory of the disease was conceived. But the value and application of the treatment by dilatation have no essential dependence whatever on the truth of the theory which led to its adoption.

It is proposed to effect dilatation of the cervical passage in various ways; by scissors, by metrotome, by sponge or lamina-ria tents, or by metallic pessaries allowed to remain for some length of time, by bougies allowed to remain only a short time, and by dilators whose use takes only a short time.

The scissors is used to open up the external os of the cervix by those who believe this part to be the seat of obstruction. An ordinary scissors will answer the purpose, but special scissors have been constructed for the operation. It is easy to snip through the vaginal portion of the cervix at each side, and thus make a free opening into the cervical cavity. The wounds heal without restoring the small size of the original os—a result which can be easily understood on considering the arrangement of the parts. The operation may be compared to what may be called making an artificial hare-lip. Were the



external os the seat of obstruction, this would be the proper operation for curing dysmenorrhœa. But I believe it is quite useless, except in the rare cases of abnormally small external os; and in them it cannot be trusted to as a cure of dysmenorrhœa should that disease complicate the malformation. If the external os is the seat of the disease, it is, of course, at least foolish to interfere with deeper parts. The plan of dilating the external os by incisions commencing at the internal os is ridiculous, both in a theoretic and practical point of view.

The great and rational attempts at a so-called radical cure, are directed to produce dilatation of the internal os of the cervix. If any particular point can be called the seat of the disease, this is the part. Many means have been, as already said, proposed and used with this object in view; but it is very doubtful whether or not any of them effects the purpose otherwise than temporarily. Cutting instruments appear at present to be the favourite tools, and it appears to me highly improbable that the result can be ultimately other than induration of the canal or passage by the resulting cicatrices. Experience, however, is the best test of their value, and may prove it; even although the theory of mechanical obstruction may at the same time be shown to be false.

The instruments for bloodless dilatation of the internal os of the cervix may be divided into two classes:—1. Those allowed to remain for at least a day. 2. Those not allowed to remain above a few minutes.

The former class includes sponge tents, laminaria-tents, metallic pessaries. The dilatation by tents is effected in the course of one or two days. The same is true of the metallic pessaries, but it is usual to allow these to remain for a long time,—that is months or years,—if they do not excite acute inflammation. Various metrotomes have been invented to incise the internal os uteri, and the operation is easily performed. Hemorrhage is not unlikely to occur, especially if the incisions, which are made laterally, are made too deep. This hemorrhage is arrested by a perchloride of iron plug in the cervix. Various injurious and useless plans are recommended to prevent, as it is said, the wound from closing, or from closing improperly.

I have used all these means of dilatation, and have been most extensively a witness, directly and indirectly, of their employment by others. The sanguine proposers of these methods have produced no satisfactory data to support their practical recommendations, and I regret to say that I have none prepared to support my own opinions. The absence of data is to a great extent justified by the impossibility of making good evidence out of them. Indeed, in the absence of any kind

of odynometry, we must be content with a distant approach to good evidence. Some physicians appear to cure their cases as well and quickly without dilatation as others do with it. Yet, in the hands of every gynecologist, there are many inveterate cases of severe dysmenorrhœa, and it is upon these alone that any dilatation should be even proposed to be tried.

My knowledge of the results of the use of the means hitherto spoken of may be summed up in a few sentences. Not unfrequently they produce a complete cure for one period; and this is no gain, because the pain (not to speak of the danger) of the operation is a set-off against it. Rarely their beneficial effect is more lasting, the cure being partial or complete. Not very rarely they have caused death either by hemorrhage or by inflammation. Very frequently they have caused cervical catarrh, endometritis, parametritis, perimetritis, ovaritis, with all their attendant ills. Frequently they have aggravated the dysmenorrhœa. In short, their use has been most unsatisfactory. So much has this been the case, that, in common with those friends on whose experience and judgment I have most reliance, I have been reluctantly forced in the meantime to discontinue the use of a most promising remedy for a most painful disease.

But the occasional apparent complete success of the plan has led me to try again the method of effecting the purpose with the least risk of doing injury, the second class of instruments for bloodless dilatation being used. This least injurious method is the oldest of all, and it has been nearly forgotten in the crowd of its modern rivals. Even of it I would not wish to be thought to speak definitively. It involves the infliction of no wound; the instrument used remains in the womb only for a few minutes at a time, and sufficient time is left between the repetitions of its use to allow the disappearance of the irritation it causes. This method certainly causes much pain; but if it do not cure, it appears to me to cause, in the very great majority of cases, no injurious inflammation.

The operation is performed between two monthly periods, and it consists in introducing as large a bougie as will easily pass (a 10 or 11 in the ordinary male urethral series). On each successive day a larger bougie is introduced, till after several introductions, say seven, a large bougie is passed (an 18 or 19 in the ordinary male urethral series). The bougie to be passed does not enter the cavity of the body of the uterus at once, but moderate pressure continued for about a minute secures its passage. The dysmenorrhœa-like pain produced is severe, and lasts for about fifteen or twenty minutes, and then the bougie is withdrawn.

All the varieties of treatment, medicinal and other, applied



in other forms of the disease are also tried in this form, and success is said occasionally to follow their use.—*Edinburgh Medical Journal*, May 1872, p. 961.

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#### 84. — ENLARGEMENT OF THE UTERUS CONSIDERED SPECIALLY WITH REFERENCE TO DIAGNOSIS.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery,  
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the Adelaide Hospital, Dublin.

[Dr. Atthill commences this paper by insisting upon the necessity for the frequent use of the uterine sound, except when contra-indicated by the existence of some special cause.]

Enlargements of the womb are met with in a very large percentage of those cases in which the symptoms are referable to the female organs of generation. Nor is this a matter of surprise when we remember the changes the uterus undergoes. In the virgin state but a couple of inches in length and an ounce or so in weight, it becomes, under the influence of pregnancy, developed into a large organ capable of containing the full-grown foetus, and weighing several pounds; consequently any circumstance which retards or prevents the return of the uterus to its normal size after delivery, may produce, as is now well known, a condition which often results in permanent enlargement, a condition to which, as I have already explained, the term “subinvolution” is applied. But, in addition to these great changes, the result of pregnancy, the uterus every month, as each catamenial period comes round, increases in weight and, probably, somewhat in size; if, from any accident or imprudence, the natural flow is then checked, that temporary increase may become permanent, an accident which, I am satisfied, is far from being of unfrequent occurrence. Here, then, at the outset, are two palpable causes of enlargement of the uterus.

But we meet with cases of enlargement of the uterus which cannot be referred to either of these classes. Women who have never been pregnant, and never have had any derangement of, or departure from, healthy menstruation, and women who, having conceived, have subsequently enjoyed uninterrupted good health for years, during which pregnancy undoubtedly did not take place, nor yet any derangement of menstruation occur, occasionally begin to suffer from symptoms referable to the uterus, and, on examination, these are found to be due to enlargement of that organ. In such cases, I believe this condition may depend on inflammation of the substance of the uterus, either of an acute or chronic character; on simple

hypertrophy of the muscular and areolar tissue of the uterus; on fibrous tumours developed in the walls of the uterus; and also, as all are aware, on the existence of intra-uterine tumours of any kind, whether they be polypi, fibrous or cancerous tumours. But, it is not my intention to enter at all on the subject of either uterine polypi or uterine tumours, except with reference to the question of diagnosis. I also purposely omit all reference to the actual existence of pregnancy, or to the retention of any of the products of conception in the uterus, as being foreign to the subject to which I wish specially to direct attention.

To recapitulate, we meet with enlargement of the uterus as the result of—

1st. Subinvolution of the uterus after labour or abortion.

2nd. Congestion of the uterus from suppression or retardation of menstruation.

3rd. Acute inflammation of the uterus, or possibly of its peritoneal covering.

4th. Chronic inflammation of the uterus.

5th. Hypertrophy of the uterus.

6th. The stimulus given to the uterus by the development in its walls of fibrous tumours.

7th. The existence of intra-uterine tumours.

1. Subinvolution of the uterus is now a well-known cause of uterine enlargement. There is no doubt but it is most likely to occur in those cases in which any form of inflammatory attack, whether it be peritonitis, metritis, or cellulitis, takes place subsequent to delivery. This fact has been pointed out by several writers. If, then, a patient has suffered from any such attack, the possible effect of it in retarding the normal reduction in the size of the uterus, which should take place within a few weeks subsequent to delivery, must be borne in mind, and we should, in such cases, carefully watch for any symptom indicating the presence of this condition. As a nearly invariable rule, profuse menstruation is the first and most prominent symptom indicating the existence of enlargement of the uterus depending on subinvolution; a symptom capable of being easily explained, when we bear in mind the fact, that not only is there under such circumstances an undue amount of blood contained in the enlarged uterine veins, but also, that the relaxed condition of the muscular tissue of the uterus favours the exudation of blood. This profuse menstruation does not always occur immediately: sometimes a month or two first elapses; but, ere long, menorrhagia shows itself, frequently of so violent a character as to assume the proportions of hemorrhage, and, on instituting an examination, the sound reveals the true state of the case by proving that the uterus is



abnormally elongated. The depth of the uterine cavity varies greatly in such cases. I have met with one instance in which it measured seven inches and upwards.

I have already stated, that the occurrence of profuse menstruation was the earliest and commonest symptom of enlargement of the uterus the result of subinvolution. This is so generally the case, that I have drawn the inference that the occurrence of profuse menstruation in cases of retroflexion of the uterus proved that the flexion was a secondary affection, the result of subinvolution of the uterus, and I still believe that as a general rule, this is correct, but there are exceptions, however, to this rule.

2. The occurrence of the enlargement of the uterus from any cause suddenly checking menstruation is by no means rare, but the opportunities of proving this to be the case do not frequently occur; for if an unmarried woman complains of fulness, of pains in the head and in the back, of a sense of weight in the pelvis, and states that menstruation has been checked by exposure to cold or by some other obvious cause during the catemenial period, we are probably satisfied that uterine congestion exists; but, we are not justified in making a vaginal examination of any kind, unless, indeed, after a protracted trial, general treatment fails to relieve her. Again, if a married woman exhibits the same train of symptoms, the possibility of pregnancy existing precludes the use of the sound. Recently, however, I had an opportunity of verifying the fact. A widow, the mother of thirteen children, in whom menstruation had been irregular for three years, had in June last, after a long interval, a return of the discharge. It ceased suddenly, and she suffered great discomfort from a distressing sensation of weight and bearing down in the pelvis, and of fulness and pain in the head. In her case the uterus was three inches in depth, while all the symptoms rapidly subsided under treatment. It may be objected that, in this case, we were ignorant as to what might have been the condition of the uterus previously; but here was a woman in the enjoyment of good health, suddenly attacked, after the abrupt checking of menstruation, with distressing symptoms, in whom the uterus was proved to be enlarged, and who was relieved of those symptoms, and of that condition by treatment. Is it not then fair to reason that the enlargement was a temporary condition, the result of uterine congestion, itself caused by the sudden checking of menstruation?

3. All modern writers agree that acute inflammation may produce enlargement of the uterus, and I believe that this may be the case, whether the patient suffers from peritonitis, metritis, or pelvic cellulitis. Of the two latter I have no doubt.

4. Chronic inflammation of the uterus being more frequently met with than the acute form, is a more common cause of enlargement. Such cases are constantly coming under observation. They are frequently found in connection with retroflexion of the uterus. Of course all are aware, that it is a disputed point whether flexions of the uterus are a cause or a result of inflammation. For my own part, I am far from denying that flexions of the uterus, but more especially retroflexion, may take place independently of inflammation, still, I am satisfied that in the majority of cases, inflammation, or at least active congestion, is the primary and principal cause of these flexions, and this opinion, which I expressed in a paper published two years ago, enlarged experience has since amply confirmed. In such cases the symptoms are often very distressing.

5. Next I shall call your attention briefly to that condition, which, for lack of a better name, I term hypertrophy of the uterus. I mean to include under this head those cases in which the whole of the uterus, or some portion of it, slowly and imperceptibly increases in size. Sometimes the cervix alone is implicated, that portion of the organ becoming elongated and thickened, or the body alone may be affected, while in other cases the body and cervix are equally engaged, and become thickened, enlarged, and frequently painful. The pain being apparently due either to hyperæsthesia of the nerves of the uterus, or to the pressure exercised on them by the hypertrophied tissue by which they are surrounded.

In these cases menstruation, as a rule, is but little altered in its character; sometimes it is slightly diminished in quantity, and not unfrequently becomes painful, but I do not remember meeting with a case in which hemorrhage was present. I am of opinion that the condition of the menstrual functions will materially aid our diagnosis in doubtful cases; for if the enlargement be due to chronic inflammation, it will most probably be lessened in quantity; if to subinvolution or to the presence of any intra-uterine tumour, it will in general be augmented; while in cases of simple hypertrophy it is seldom altered, at least in any great degree.

The pathology of this form of uterine enlargement is very obscure; the fibres composing the muscular tissue of the uterus appear to be elongated and thickened, while there is also hypertrophy of the areolar tissue. Both conditions may have their origin in a low form of inflammation which at the time escaped observation; but we cannot, in the present state of our knowledge, say, why in a certain case the cervix uteri elongates and enlarges, till by its very size and weight it irritates and causes distress; while, at the same time, in another the body



and fundus of the uterus participating in the unhealthy condition of the cervix, become heavy and elongated; and in another, seem to remain in their normal condition. Excessive indulgence in sexual intercourse has been set down as a cause of enlargement and hypertrophy of the cervix, but I doubt this much. In my own practice, the case which of all others gave rise to the greatest amount of suffering and distress, occurred in an unmarried woman. Miss——, æt. 40, came under my care nearly two years ago. She stated that about seven years previously she, for the first time, experienced pain in the back and over the pubes, and not long after a sense of weight in the pelvis, and I saw that her suffering had ever since gradually increased. When I saw her she could not sit up for any length of time, and walking, even a short distance, caused much distress; she also complained of a troublesome itching at the vulva. On instituting a vaginal examination, the cervix was found to be elongated to the extent of at least an inch. It was thickened and hypertrophied, the supra-vaginal portion evidently participating in the abnormal condition of the organ. Menstruation continued to be performed regularly, but it was attended with much pain. She has ever since been under observation, though I have long ago abandoned any active treatment. This lady has been treated by leeching, by blistering, by the administration of the iodide of potassium and of the bichloride of mercury, and it is hard to say which of these did the least amount of good. The uterus has steadily increased in size, evenly and universally, and with its increase so have her sufferings increased, till now she has become a complete invalid. In her case I at one time, before the body of the uterus became so manifestly enlarged, entertained the idea that amputation of the cervix might, by removing the cause of local vaginal irritation, be productive of benefit. But, as the operation certainly was not entirely free from risk, and as I became satisfied that the supra-vaginal portion of the uterus also participated in the diseased condition, I abandoned the idea. In this case I feel perfectly satisfied that the hypertrophy commenced at a very early age and gradually increased; it bears a strong resemblance to one recorded in Vol. ii. of the work on *Diseases of Women*, by Bernutz and Goupil, published by the Sydenham Society. Another case of hypertrophy of the cervix in an unmarried woman has since come under my observation. She is a dressmaker, æt. 28, an industrious woman, sitting at work upwards of twelve hours a day. She complained of weight in the pelvis and of bearing down. She also suffered from the most obstinate constipation. Menstruation was regular, but generally accompanied by pain. On making an examination the os uteri was found to rest on the perineum;

the cervix was elongated and thickened, and the fundus slightly enlarged. This woman would not come into hospital, and consequently I have had no opportunity of trying the effects of treatment, from which, in truth, I would anticipate but little benefit.

Any person who has read the work just mentioned will at once see that the condition I am now referring to is very similar, if not analogous, to that termed by M. Huguier, "hypertrophic allongement" of the uterus, a condition which he divides into two classes—namely, sub-vaginal and supra-vaginal, a division the actual value of which I do not highly appreciate. I am inclined to the opinion that, although we may have enlargement of the body of the uterus without the cervix being engaged, the cervix is never enlarged for any length of time without the supra-vaginal portion of the organ becoming implicated in the disease. I also believe that not a few of the cases recorded by M. Huguier were cases of subinvolution of the uterus following delivery, and not of the condition which I have termed hypertrophy.

But, in addition to these cases of hypertrophy with elongation of the cervix or of the body of the uterus, or of both, we meet with cases in which there is no elongation, but the very reverse. We sometimes find the cervix shortened, drawn up, as it were, into the body of the uterus, and sometimes disappearing altogether. In such instances the body of the uterus assumes a globular form. This form of enlargement gives rise to considerable distress, and it seems specially to cause intractable irritation of the bladder. In one case, which was for years occasionally under my observation, this symptom was the prominent one, and that for which the patient sought relief.

There is no form of uterine disease in which so little good can be effected by treatment as that to which I am now referring. If the body of the uterus be engaged, it seems nearly useless. If, however, we are satisfied that the cervix only is affected, amputation may be resorted to with advantage; or possibly local depletion and subsequently the repeated application of Dr. Greenhalgh's iodized cotton may effect some good.

It remains for me to allude, and I shall do so very briefly, to that form of uterine enlargement in which the organ is stimulated, and increases in size, from the presence of a fibrous tumour embedded in, or growing from, some portion of its walls. Cases are recorded in which a fibrous tumour of very small size, perhaps not larger than a nut, so stimulated the uterus that it increased to five or six times its normal size, the cavity too being proportionally elongated. These cases are most perplexing, a *post-mortem* examination alone being capable



of revealing their true nature. Fortunately they are not of frequent occurrence. I have not myself met with any case in which I was satisfied of their existence. In the great majority of instances a fibrous tumour sooner or latter will bulge into the cavity of the uterus, or project out on the peritoneal surface. In either case the tendency of the disease is to render menstruation more profuse; while in that form of enlargement depending on hypertrophy of the fibrous tissue of the uterus, and which is the only form liable to be confounded with the one now under consideration, menstruation, if interfered with at all, is more likely to be diminished than increased. The subject of fibrous tumours of the uterus does not come within the scope of the present lecture. I wish, however, to draw attention to those cases, of by no means infrequent occurrence, where enormous fibrous growths exist in which the womb is imbedded and almost lost. These cases have over and over again been mistaken for ovarian tumours, a mistake which the use of the uterine sound may help us to avoid. It tells us not only what is the length of the uterine cavity, but also whether the uterus is free or imbedded in the tumour.

Now, as to diagnosis. I have already stated that the sound and that alone enables us to decide as to whether the uterus be enlarged or not, but it affords us no clue as to the cause of the enlargement. A few general rules, however, if they do not enable us to give a positive diagnosis, will at least facilitate materially our decision as to the nature of any case. Thus, if we meet with an enlarged uterus in a woman who has aborted or been delivered at the full time, even though a considerable interval has elapsed, the probability is in favour of the enlargement being dependent on subinvolution, and this opinion will be confirmed if, as is nearly always the case, menorrhagia be present. If again metritis, pelvic cellulitis, or peritonitis be present or have occurred recently, the inflammatory action is fully sufficient to account for the condition of the uterus, and it should be always borne in mind that it does not follow that the enlargement will disappear with the subsidence of the inflammation; so again, in other cases, we should ascertain if menstruation has been checked or suppressed, and if symptoms referable to the uterus have followed on this; or if again, pain in the back and over the pubes was first noticed, menstruation being subsequently lessened or suppressed: in the former case we are likely to find that the enlargement depends on congestion, in the latter on chronic inflammation, while hypertrophy steals on gradually, menstruation being seldom interfered with; and it is of no small importance to decide to which cause the enlargement is due, for while much may be done to relieve the sufferings caused by enlargement of the

uterus the result of chronic inflammation, treatment seems utterly powerless in alleviating those produced by simple hypertrophy of the uterus. It is, indeed, a nearly hopeless ailment, one not likely to destroy life, but to render it a burden. Then, again, if we have menorrhagia in cases of enlarged uterus, unconnected with any of the causes noticed, we may expect to meet with intra-uterine polypus, or fibrous tumours. It will then be our duty to clear up the doubt which exists, by dilating the cervix and exploring the interior of the uterus.

As I have called your attention to the subject of enlargement of the uterus, with the hope that I may aid you in arriving at a correct diagnosis in cases in which that condition exists, I shall not enter at any length into their treatment; that of subinvolution was fully discussed on a previous occasion, and I must refer you to what was then said on the subject.

In cases of enlargement following sudden suppression of menstruation, the administration of saline purgatives, and subsequently of the bromide and iodide of potassium conjointly in full doses, will generally, if the case be recent, prove sufficient; but should it be neglected in the early stages, it will probably pass into the condition of chronic inflammation, a condition over which medicines possess little influence. The prolonged use of the bichloride of mercury, in small doses, has been recommended in these cases, but in my own practice I have not been satisfied that it produced any good effect. I have seen, I think, more benefit result from local depletion by puncturing the cervix uteri, as recommended by Dr. Hall, of Brighton, than from anything else, and I think it is a mode of treatment deserving a fair trial. To be of use this must be repeated frequently at intervals of about five days. The application, to the verge of the anus, of two or three leeches, immediately after the termination of a menstrual period, where menorrhagia is present in connexion with a relaxed and engorged uterus, also often proves beneficial. Both these methods act by relieving the congested condition of that organ, and thus facilitating its contraction. In conjunction with this treatment, I recommend the administration of strychnia with the addition of dilute nitric acid, or, if the patient be anæmic, with the tincture of the perchloride of iron. Strychnia is the most valuable medicine we possess in case of menorrhagia in connexion with a relaxed atonic condition of the uterus. It is, however, contra-indicated in cases of chronic inflammation, unless that condition be first relieved by local depletion.

In cases where the uterus has become enlarged and hardened, as the result of chronic inflammation, the use of the waters of Kreuznach seems to have a very beneficial effect, and if the



patient's means are such as to admit of her visiting that place, a trial should be made. As to hypertrophy of the uterus, treatment is seldom likely to effect good.

In cases of enlargement of the uterus from inflammation of an acute character, I believe rest, the exhibition of opium, and warm poultices over the abdomen to be the means on which we should rely; depletion, if practised at all, should be in a limited degree by the application of a few leeches. Mercury I consider to be not only useless but actually deleterious.—*Medical Press and Circular*, Sept. 4, 1872, p. 189.

### 85.—ON PROCIDENTIA OF THE UTERUS, AND OF THE BLADDER.

By DR. EVORY KENNEDY, late Master of the Dublin Lying-in Hospital.

*Obstinate Procidentia of Uterus cured by Actual Caution.*—Mary Burke, æt. 60, a widow with six children, had obstinate procidentia of the uterus, and could not retain any form of pessary. She suffered much distress; her general health was deranged; she was dyspeptic, and had lumbar pains. She was confined to the horizontal position, with the hips well raised, for a fortnight. The ulcers healed. The uterus was placed *in situ*, and astringent injections were used. The actual cautery was then applied about an inch and a-half from the vulva round the surface of the vagina for the extent of about half an inch. My clinical clerk, who was somewhat of the Gil Blas school, in criticising his master, adds that the old woman was so indignant at this operation, which, for obvious reasons, it was deemed more considerate not to explain beforehand, that she left the hospital in a fit of indignation, because a hot iron had been applied to her inside, as she said, without saying, "With your leave, or by your leave." She eventually permitted the treatment to be followed up by the application of nitrate of silver; and the report is thus continued by my clinical critic. At the expiration of two months, she was able to go about again. The uterus was *in situ*; and she had derived much benefit to her general health and condition, and a distressing pain, descending down the front of the thigh, from which she had constantly suffered, had disappeared. The cicatrix formed by the eschar prevented the descent of the os. He adds, however, this caustic comment on my operation: "She certainly got a regular touching up."

*Procidentia Vesicæ treated by Vaginal Mould.*—In February 1839, Catherine Sullivan was admitted with complete procidentia of the bladder, the result of a tedious and instrumental delivery of her first child. She had had two pregnancies, and

suffered from prolapsus ; but the protrusion occurred suddenly about two years before admission, whilst she was lifting a feather-bed. She was treated by a variety of pessaries, and the actual cautery was applied to the interior of the vagina ; but the protrusion recurred despite of all. She was kept for some time in the horizontal posture. A cast of the vagina was taken in wax, with the bladder restored to its natural position. A plaster of Paris mould was made from it ; and in this a caoutchouc instrument was moulded. This she wore with great comfort and convenience. It should be mentioned that the difficulties in this case were added to by the existence of a growth supposed to be an exostosis, springing from the interior of the sacrum, and encroaching on the inlet of the pelvis.—*Medical Press and Circular*, Sept. 25, 1872, p. 253.

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#### 86.—ON FIBROUS TUMOURS OF THE UTERUS.

By Dr. GEORGE H. KIDD, President of the Dublin Obstetrical Society ; Obstetric Surgeon to the Coombe Lying-in Hospital, Dublin.

In speaking of fibrous tumours of the uterus I have no intention of entering into the literature of the subject, or discussing the pathology and etiology of these growths. I mean simply to bring before you some account of my own personal experience in reference to them, and to do so as briefly as possible. You all know that in the books fibrous tumours are spoken of as of three classes. In my remarks I shall follow this order. We have fibrous tumours found on the outer surface of the uterus, or sub-peritoneal tumours. We have them found in the substance of the uterus, or intramural tumours, and we have them found in the interior of the uterus, or intra-uterine tumours. The sub-peritoneal tumours are, as we know, the largest, and are very frequently numerous. There are cases recorded of tumours of this kind 70lbs. in weight. It has never been my lot to meet with a tumour of that size. Once I saw one with Dr. Denham upwards of 40lbs. in weight, and it presented another peculiar character of these tumours—the whole mass was formed of an agglomeration of tumours. The chief importance of sub-peritoneal tumours arises from their size and their position. The remark has been made by Cruveilhier that the sub-peritoneal tumour is not accompanied by hemorrhage ; but a doubt has been thrown upon the absolute correctness of this statement. At present I am not aware that we have had an actual dissection of a case where a sub-peritoneal tumour was accompanied with hemorrhage. The specimen I now lay before you seems to me one of very pecu-



iar interest, and well calculated to throw light on this question. It is a uterus having on its outer surface a very small specimen of a sub-peritoneal tumour, not larger than a large-sized filbert; but there is a soft mucous polypus growing from the inner surface, at the fundus, and projecting into the cavity of the uterus, and there is also at the os-internum a number of cystic polypi. It was a case where we had a sub-peritoneal tumour combined with intra-uterine polypi, in which the fibrous tumour might have been recognised during life and the polypi have remained undetected. I can very well imagine that in such a case as this you might have a sub-peritoneal tumour complicated with hemorrhage due to the polypus and not to the tumour. This, then, would afford a satisfactory explanation for the occurrence of hemorrhage in connexion with sub-peritoneal tumours. I have no history of that case. It was a specimen brought to the Pathological Society with others, taken out of the same body; the other specimens were those that attracted attention, but the gentleman who exhibited them was so good as to give me the specimen now before you. I could not obtain any history of the uterine symptoms during life. The chief importance, however, of these sub-peritoneal tumours is from the mechanical inconvenience they cause. These tumours, when they grow from the upper part of the uterus and rise into the abdomen, are of little importance; but they sometimes grow from the lower part, and are pressed down into the true pelvis, where they are surrounded by bone, and may give rise to very serious inconvenience indeed. I have notes of a very remarkable tumour of this kind that I had the opportunity of observing from the beginning to the end. It was the case of an unmarried lady who consulted me so long ago as 1852 for a tumour she had discovered in the abdomen. It was then about the size of a goose's egg. The late Dr. Charles Johnson saw the case with me. A second tumour developed itself as I watched the progress of the case. The tumour increased, and the abdomen became enlarged, apparently occupied by one mass, but two tumours could still be traced. In 1859 the abdomen was as large as in the seventh or eighth month of pregnancy; and at this period a portion of the tumour could be felt by a finger in the vagina lying behind the uterus and pushing that organ upwards and forwards. Attention was drawn to it by the patient herself, who found a greatly increasing difficulty in using an enema.

On examination, at this time, I found a tumour of stony hardness; and on passing a sound into the uterus, I found this organ of the normal length and, I have already mentioned, pushed upwards and forwards. The tumour behind the uterus increased in size so much, that there was a difficulty in getting

the bowels to act, and at each menstrual period there was great pain in the tumour, and she suffered from great sickness of stomach. At no time was there uterine hemorrhage.

In 1861. Dr. Churchill saw this lady. The lower tumour had now increased, so as to throw the uterus quite out of reach of the finger. The whole of the upper part of the pelvis was occupied by the large hard solid mass, so tightly wedged into the pelvis as to be quite immovable; and the tumour rose in the abdomen to midway between the umbilicus and xiphoid cartilage. The constipated bowels and difficulty of defecation continued; the sickness of stomach was not confined to the menstrual periods; menstruation was regular as to time and quantity, but very painful. But the greatest source of complaint was now excessive pain along the course of the great sciatic nerve of the right side. Dr. Churchill suggested the use of an air-pessary strongly inflated, to bear the pressure of the tumour off the sacral plexus of nerves, and to gradually raise the tumour off the rectum. This was used for some months, and afforded considerable relief. The lady now went to the County Wexford, and was under the care of Dr. Isdell, who wrote to me that she had a fibrous tumour so completely blocking up the pelvis, that he thought defecation would soon be impossible.

This is a very marked example of the mechanical inconvenience arising from this class of tumours. The pain in the sciatic nerve was intense, and it was something frightful to witness her agony arising from it. The subsequent history of the case is still more remarkable and more interesting.

At the end of 1863, menstruation became irregular, and she occasionally passed "flesh-like masses," which she believed to have been coagula, as I presume they were. Menstruation now ceased, and the abdomen gradually diminished in size; the pain and sickness of stomach ceased; and the abdominal tumour disappeared without its going being in any way noticed, except from the diminution of size, which was so gradual, as to be only known by the result, and not by the process. The general health was greatly improved.

On 26th of June, 1867, I saw this lady, and examined her carefully. I could not detect any tumour in the abdomen. On passing my finger into the vagina, I found a firm round tumour in Douglas' space—movable, hard, but yielding slightly to the finger on pressure. The uterus was easily felt, pushed a little forwards by the tumour, but nearly in its normal position. The vaginal portion of the cervix and the os were quite defined. The uterus moved freely, and independently of the tumour. The patient had not known of the existence of this part of the tumour, and believed the whole had disappeared.



This was one of the most remarkable cases I have ever known of the disappearance of a fibrous tumour. I saw the case from its beginning, and the lady is still alive and still under my observation, and is herself not aware of the existence of any tumour, and I believe there is now none at all.

The pressure here was upon the rectum and the sacral plexus of nerves, and the pessary afforded the patient great relief. In other instances the pressure is upon the urethra and upon the neck of the bladder.

The following case is an example of this:—A. D., an unmarried woman between forty and fifty years of age, applied as an out-patient at the Coombe Hospital, in January, 1871. She stated that during the last fourteen months she had frequently suffered from retention of urine, and was obliged to have a catheter passed five times. On examination, a fibrous tumour was found growing from the posterior wall of the uterus, lying in the concavity of the sacrum, and forcing the uterus against the neck of the bladder. The tumour was fixed in this position. I tried first to move it with my hand by placing my finger in the vagina and pressing it up. I got the woman on her knees and elbows for this purpose, but could not move it. I then adopted a method I have on previous occasions described to this society. I placed one of Barnes' India-rubber bags in the rectum and forced water into it by means of a syringe. As I forced the water into the bag it gradually lifted the tumour out of the pelvis, got it up above the brim, and allowed the neck of the uterus to come down into its normal position. I then removed the bag, and placed in the vagina one of Hodge's pessaries. The woman complained, next day, that she had a tumour in the abdomen, which she had not had before. I have seen this woman frequently, the last time I saw her being the 29th of January, this year—a year and a few days since I lifted the tumour. It was still in the abdomen, had not returned into the pelvis, and she never had any further difficulty in passing water.

I have seen a very considerable number of cases of this kind. They illustrate a remarkable circumstance, common to many other forms of tumour besides those under consideration—that the inconvenience arising from them, although mechanical, is intermittent. This woman suffered from retention of urine, but by means of baths she got relief, except on five occasions when the catheter had to be used. I think it is a remark of Dr. Charles Johnson, that a permanent obstruction of the rectum has never been recorded as arising from the pressure of fibrous tumour, but you may have at intervals serious obstruction in the rectum. The same takes place at the neck of the bladder. It seems to me that there are three circumstances that contri-

bute to this. I think these tumours often become increased at the menstrual period. The first case, where pain in the sciatic nerve was always aggravated at the menstrual period, is a case of that kind. Another circumstance influencing them is the condition of the bowels. Where you have the bowels distended, the tumours become more prominent, or may be forced lower down into the pelvis, and in that way pressed much more on the lower parts. These tumours are also liable to become œdematous by being in some degree infiltrated with serum.

The increase of pressure by flatulent distention of the bowels was very forcibly brought under my observation in a case I saw with Dr. Gordon and the late Dr. Beatty. It was not, however, I believe, a fibrous tumour. In Dr. Gordon's opinion it was a case of ovarian tumour; but, as it illustrates the point to which I refer, I may, perhaps, be allowed to speak of it. The case was that of a lady who had been under Dr. Gordon's observation for a considerable time for a tumour occupying the left and lower part of the abdomen. In some way the bowels became obstructed, and she was for a very considerable period unable to pass anything from them. The abdomen became enormously distended, and we had all the symptoms of intestinal obstruction. The case was one in which ovariotomy or incision for exploration was absolutely impracticable in our opinion. We felt that if we once made an incision into the abdomen, and the intestines escaped, we never should be able to get them back again, so great was their flatulent distention. After trying many means to obtain relief, we punctured the bowels. We passed a fine trocar into the transverse arch of the colon. A great escape of gas took place. The candle having been brought near, the gas took fire, and a blue flame was formed two or three inches in length. The lady suffered no inconvenience from the tapping. The next day the bowels acted freely, and continued to act for some time, until she gradually sank and died from other causes. It shows that flatulent distention of the bowels may cause a tumour to press on neighbouring organs.

The next point to which I desire to call your attention is the disappearance of fibrous tumours. In the case of the sub-peritoneal tumour that I have related, I believe the tumour disappeared by absorbtion. There was no escape of anything that could account for its disappearance. It is doubted whether these fibrous tumours are capable of being absorbed, but I believe this case fully proves it. Another change these tumours undergo is that of calcification, especially after menstruation has ceased, when they sometimes become converted into a cretaceous mass. Reasoning on this, the use of chloride



of calcium was suggested, and Dr. M'Clintock speaks highly of it. The practice is one I have been in the habit of following for a long time. I do not know that I can give you an absolute proof of the diminution of tumours from the use of chloride of calcium, but the patients express themselves very much relieved by the use of it, and it has given more comfort than any other treatment I have seen adopted.

The next class of tumours are the intra-mural tumours. I have not much experience of my own to record with regard to intra-mural tumours. I have seen many of them, but I do not know that I have anything worthy of detaining the Society to say with regard to them. In my experience they are the least amenable to treatment, while they are the most serious, perhaps, to the patient. They cause sometimes great hemorrhage; at other times there is no hemorrhage from them. Of this I had a remarkable example—a case of intra-mural tumour, where I had an opportunity of dissecting the patient, and where the presence of the tumour was fully established, and yet there was no history of hemorrhage, even though the cavity of the uterus was very much enlarged. In other cases not nearly of that size, and where the cavity of the uterus was not so much enlarged, the hemorrhage was excessive. I believe the difference is owing to this—when the tumour is near the mucous surface, and presses on it, you have hemorrhage; when it is in the substance of the wall of the uterus, and grows towards the outer surface, there will probably be no hemorrhage. The the cavity of the uterus may be enlarged in both cases, but not so much in the latter. When you pass a sound into uterus you find it double or treble its normal length, and sometimes very tortuous too. Many methods have been suggested for the treatment of these cases—incision into the capsule of the tumour, gouging, incision of the neck of the uterus, &c.

I have never seen a case where it seemed desirable to adopt these methods of treatment; and, indeed, they seem more likely to be useful in cases of intra-uterine tumours, which we now know can be removed. In some cases I have dilated the uterus, and applied nitric acid. This is often beneficial; the tumour is checked in its growth, and the hemorrhage very much lessened. After a time it may return, and I have in such cases applied the nitric acid a second and even a third time. Nitrate of silver, if introduced into the uterus and left there, will check the hemorrhage; and the injection of perchloride of iron has also been used for the same purpose. That, in my experience, is the least useful and the most dangerous treatment. I have seen very serious results from perchloride of iron introduced for these tumours. The last case in which I tried it proved fatal. The woman got a low form of metritis

and died. Nitric acid does not produce nearly the same amount of irritation, and it is more beneficial. The palliative treatment is of great importance in these cases, and the best form of it is plugging.

I wish to stop here to make a few remarks on plugging. It has become the fashion of late to plug with cotton, and I have fallen into the way with others. Cotton possesses some very great advantages both for introduction and removal. You take a sheet of French wadding, cut a long stripe off it, and pack it carefully in the vagina. It is not, in my experience, an efficient plug. It is a peculiarity of cotton, that as you wet it, it becomes contracted and occupies a smaller space than it did at first. When placed in the vagina it becomes wet with blood, contracts greatly, and leaves the vagina free enough to allow a discharge of blood. This may be obviated to a certain extent by wetting the cotton before introducing it. Even then it is not so good as the old-fashioned sponge plug. Sponge is the reverse of cotton; it expands when wet, and fills the vagina more completely. When the cotton plug is wet the blood will trickle past it; whereas the blood gets into the interstices of the sponge and coagulates, and the hemorrhage is arrested. I have, therefore, nearly given up cotton, as a material for plugging, in cases of these fibrous tumours. There is one way in which cotton can be rendered more efficient, and that is by saturating the first portion introduced with perchloride of iron; this produces coagulation, and the bleeding is checked. And a more efficient method, when it can be managed, is to plug the os uteri itself with a piece of prepared sponge, and it will give the patient much less inconvenience, and be a very effectual plug. Sometimes these intra-mural tumours are in such places that they can be removed. I have one here which was situated on the posterior lip of the uterus, and I removed it with an ecraseur.

Intra-uterine tumours may grow from any part of the uterus, but I never met with one growing in the canal of the cervix. They grow, in my experience, chiefly from the cavity or body of the uterus, and they may attain a very formidable size. There is a point with regard to these tumours that I have observed, and which I have never seen noticed by writers, and it seems to be one of considerable practical interest and importance. The tumour in one case, grew from the body of the uterus, just above the os-internum, and it lay in the canal of the cervix. It grew from the anterior wall of the uterus, and pressed out the posterior wall as you see it in this drawing. When a finger was passed into the vagina, the tumour could be felt like a retro-flexed uterus, the posterior wall of the uterus was so bulged out. My present experience of these tumours, where you have one side of the uterus bulged out, is that



the pedicle of the tumour is attached opposite to the bulging part of the uterus. I have seen a number of these cases. In the case to which the drawing refers there was great difficulty in introducing sea-tangle, from the position of the pedicle and the shape of the tumour. When it was attempted to pass the tangle where the side of the uterus was straight, it hitched on the pedicle. At the other side it was difficult to get the tangle in, because of the convexity of the tumour. So frequently have I met with this circumstance, that the tumour bulges out the wall of the uterus opposite to where it is attached, that I have ventured to predict where the attachment of the tumour would be found, and I have been, up to the present, invariably right. The tumour I now exhibit presents this feature in a remarkable degree. It was removed from a patient of Dr. Ringland's. She had been greatly reduced by hemorrhage; and when she came under our observation the fundus of the uterus could scarcely be felt in the abdomen, but we could feel, when we passed the finger into the vagina, a large tumour under the body of the pubis. We dilated the uterus and explored it, and removed the tumours. The removal of these tumours is done by dilating the os, and detaching them with the ecraseur.

Some years ago I brought under the attention of the Society some cases of intra-uterine polypus. I gave a diagram of a case where the wall of the uterus was bulged out, and my impression then was that the polypus grew from the part where it was bulged out. My subsequent experience leads me to believe that this was an erroneous observation and that it grew at the opposite point.

The tumour I exhibit in this glass was the first large-sized one I ever removed. It was the case of a lady of very considerable position, and it was the first time I had an opportunity of trying my hand on these tumours in that rank of life. In my operations for small tumours and polypi, I was in the habit of using for an ecraseur a soft iron wire, and I thought there were advantages in using it. I attempted to remove this tumour with a soft iron wire and failed completely. My patient was very courageous and very determined. She waited a few weeks, came back to town, and I tried again, and succeeded at the first effort in removing the tumour. I succeeded because I used a steel wire, a piece of piano string, in fact, which answered the purpose admirably. The long neck of the uterus is dilated with sea-tangle until the finger can go up, and the tumour is caught and fixed by the vulsellum. The soft wire when passed up assumes the shape of the canal of the cervix, and it is difficult to expand it again so as to get it round a large tumour. By using a steel wire you can compress it to

get it through the narrow os, and when you get it up it expands by its own elasticity, and you slip it over the tumour with ease. I took out the tumour in three portions. It was so broad I could not get the wire completely round it. I dragged it down by the vulsellum, took away a piece, and this gave room to take away more, and in this way the whole was removed at the one operation. I have had repeated opportunities of seeing this lady, and she is in good health, and her uterus is now normal in size and position, and menstruation is very moderate. The next large tumour I attempted to remove was this one, and I was assisted in the operation by Dr. Beatty. This grew from the fundus of the uterus, which was even and regular. It was very soon after I had failed in removing the other tumour that I was called on to operate for this. It weighed seven ounces, the other one being two and a half ounces. I kept the woman in hospital six weeks before I could make up my mind to attempt the operation. So deterred was I by the difficulty I had experienced in using the soft wire. Turning it over in my mind, Dr. Barnes' suggestion to use steel wire for dividing the foetal head in certain cases of difficult labour occurred to me, and I determined to try it, and with it I succeeded in snaring the tumour and taking it away at the first attempt. I had an opportunity yesterday of hearing of this patient. She had been nearly dead when she came into hospital. In fact she had been taken out of her bed and laid on the floor "to die easy" according to the superstition of the country, when Dr. Clarke, of Bailieboro', who was passing by, went into the house, plugged the vagina, and checked the hemorrhage, and then had her sent to me. She is now perfectly recovered, and most anxious to return to the hospital to learn to be a nurse.

The next specimen I have to exhibit is a tumour which resembled very closely the one I removed in three portions. It was taken from a patient of Dr. Brady's; and he and Dr. Ivory Kennedy, assisted at the operation. Dr. Kennedy said he saw this lady at the time of the last menstruation, and he verily believed she could not have survived such another. She is now quite able to attend to her business, and I believe is very well. The operation was performed six or eight months ago, and the case was not a promising one. When I passed my finger into the uterus, I found a large mass growing from the posterior wall, and another tumour growing in the anterior wall. We removed, in three portions, the mass growing from the posterior wall, which was all that was prominent in the uterus, but we left that which was imbedded in the anterior wall. Her first menstruation after the operation was a profuse one, and I had to plug; but her subsequent menstrual discharges



have been very moderate. She is a young woman, and is every day at her place of business in a large establishment in this city.

In that case I ventured to predict before the operation, from the peculiar shape of the uterus, where we should find the pedicle. There is only one other point as to these intra-uterine tumours which I wish to speak of. Sometimes they are removed by nature. Here is one expelled out of the uterus without any interference of art. Here is another that was also expelled in the same way. In one of these cases, the lady came from Cork, and placed herself under my care. I found signs of a considerable tumour in the interior of the uterus, and I made arrangements for the removal of it. An appointment was made for the operation, and some friends were requested to join me. I happened to have a very fatiguing case the night before, and begged to be allowed to postpone the operation. It was accordingly put off. That night the lady had a rigor. The next morning her pulse was 150, and she was in a high fever; and she remained in this state for a fortnight, and at its termination this mass was expelled. There was no return of hemorrhage, and she went home to the country comparatively well, and the uterus greatly reduced in size.—*Dublin Journal of Medical Science*, Aug. 1872, p. 132.

#### 87.—CASE OF IMBEDDED UTERINE FIBROID.

By Dr. JAMES WHITEFORD, B.A., Greenock.

It is hoped that an account of the following case may be interesting as illustrative of the plan proposed by Dr. Matthews Duncan, for the treatment of imbedded uterine fibroid tumours (*Retrospect*, vol. lv., p. 344), and of which I published a case in February, 1870.

The history of this case, moreover, is somewhat remarkable, from the long period of time over which the patient suffered from the hemorrhage, caused by the presence of the tumour, which, nevertheless, showed no signs of undergoing self-enucleation—a period passing even that reported by the physician, St. Luke, of the “woman who had an issue of blood twelve years, and had spent all her living upon physicians, neither could be healed of any.”

Mrs. F.F., æt. forty-six—married for twenty-four years—a native of Greenock, had emigrated with her husband, and was confined of her second child at seven months, in Rock County, Wisconsin, U.S., in April, 1850. Six months after this event she returned to Greenock, where she has since continued to reside. She states that since that confinement she has never

dried up. During the voyage back, she was soaking in blood the whole time, and she never expected to reach home alive, such was her prostration. For ten years afterwards she was almost continuously confined to bed, and frequently fainted on attempting to get up. About a year after her return—during which time she had been under treatment—the existence of some tumour was suspected to be the cause of the bleeding, and she was, therefore, recommended by the late Dr. Mackie to see the late Professor Simpson, but she never went, as, indeed, it was almost impossible for her to be moved. Cold sitz baths, and injections of cold water, gallic acid, ergot, and other hæmostatics were assiduously employed to check the hemorrhage, but all to no purpose. From the constancy of the discharge during all those years, she could not tell when a monthly period came; but it occasionally increased to an alarming extent, (probably thus marking the regular menstrual period), and frequently on those occasions she was believed to be dead, as no pulse could be felt at the wrist, and the heart's impulse alone enabled those beside her to know that she still lived. About seven or eight years ago, she began to suffer from severe pain in the left breast, for the relief of which she took laudanum, and this she continued afterwards to use, frequently to the extent of an ounce and a half daily. It is singular that, although the constant dribbling continued, still it was notably less; nor were the severe hemorrhages ever so alarming as before she began to use laudanum.

In April, last year, she first came under my notice, and an examination of the womb at once revealed the presence of a small vascular polypus, projecting from the os. At the same time the body of the organ felt heavier and larger, and the cervix was soft and natural. The removal of the small polypus was not followed by any diminution of the discharge, which consisted of thin watery blood. In June, I dilated the os uteri, by incising freely the cervix with a pair of seissors on both sides, and afterwards introducing as large a sponge tent as could be got in. On exploring the cavity, which was more readily effected by pulling the uterus well down, by means of a vulsella applied to the lip, a tolerably large smooth projection could be felt occupying the whole of the fundus, and not approaching the cervix. A free incision was at once made across this, about two inches in length, as nearly as I could judge, and another similar incision at an angle to this, by means of a straight bistoury, guarded with lint to within half an inch of the point. A solution of perchloride of iron was at once injected, and a plug of lint, soaked in the same, was introduced up to the cervix, until the following morning. This put an end to the bleeding, which was not very considerable. A smart



attack of metritis followed this operation, and I was obliged to allow her to resume the use of the laudanum, which I had stopped from the commencement of my attendance upon her. In a few days the irritation subsided, and a foetid watery discharge began, but no further bleeding was experienced from that day. The foetor of the discharge was corrected almost entirely in three weeks, and her condition greatly improved, so that I ceased to visit her in July, requesting her to let me know if any further bleeding should occur. The watery discharge continued to a slight extent, but nothing occurred to mark the presence of the tumour until the beginning of December, when regular labour pains came on (but without further discharge than the serous oozing), and the patient herself states that these were more severe than in either of her actual labours. After suffering these on her knees all day—during which she did not send for me—something like a round smooth ball was felt to be in the vagina, and the patient herself pulled it away. This proved to be a flattened ovoidal mass, about three or four inches long, by about one inch in thickness. In about three weeks after this a similar labour brought away another tumour, rather larger than the former, but very like it. Only a very little blood was lost after the former of these, and none at all after the second. The patient after washing the tumours and keeping them rolled up for me, finding that they had begun to smell by the time she thought it safe to venture out, then threw them into the fire. The serous discharge dried up in less than a week: and exactly one month after the second tumour came away, a regular menstrual period came on, which lasted three days. She has since found the menstrual function quite natural; and her general health is quite restored.—*Glasgow Medical Journal*, May 1872, p. 321.

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## 88.—TWO CASES OF CHRONIC INVERSION OF THE UTERUS,

WITH DESCRIPTION OF NEW APPARATUS FOR REDUCTION.

By Dr. J. BRAXTON HICKS, Physician-Accoucheur to Guy's Hospital.

When inversion of the uterus has continued after the commencement of the involution process, the difficulties attending its reduction increase rapidly as that process approaches its completion; and when involution has been completed, and the organ has contracted to its normal size, the obstacle to its restitution is very considerable indeed. Besides this there is the risk that the opposed surfaces of the peritoneum, turned nward, will become adherent, and thus render reduction impos-

sible. Hence it is always extremely desirable to reduce the uterus at the earliest possible period.

The first case narrated below is a painful example of the effects of the neglect of this rule; and in the second, although it was cured, yet a large amount of treatment might have been avoided had the case been attended at an early date after its occurrence.

*Case 1.*—Mrs. —, primipara, about twenty-five years old. The labour was natural, excepting that the placenta was adherent, requiring removal, which was done with some trouble. She went on well for ten days, when something came down through the vulva. The nurse called the attention of the medical attendant to the fact; but, without examining, he said it would all come right. It, however, did not cause much distress; but after a few months she began to suffer menorrhagia, which by the time she came under my care had completely blanched her. About the tenth month after delivery, she fell into the hands of another medical man, who examined her and thought it a case of polypus. He asked me to see it, and I found a body hanging from the upper part of the vagina like a polypus; but upon careful examination all round its attachments by the sound, nothing like the opening into the cervix could be found, nor, indeed, anything of the uterus above the pubis; but the cup-like condition at the position of its attachment could be felt from the exterior, showing clearly that it was the inverted uterus which was felt in the vagina. I made an attempt to reduce it, after placing the patient under chloroform. Compression of the body by the hand in the vagina combined with upward pressure, counteracted by the external support, was, after about half an hour, so far effectual as to restore the cervix, but not the body of the uterus. I then added additional force, but without result, except the production of very severe collapse, upon which the chloroform was withdrawn and the attempt given up. I then, as soon as she was recovered, passed an old-shaped stethoscope into the vagina, and placed the cupped end against the fundus uteri, the ear-end remaining outside the vulva. This end was pressed upward by a T-bandage, and removed twenty-four hours afterwards for a short time and replaced. This pressure was kept up for two days, when she was again placed under chloroform and hand-pressure employed as before. The uterus was partially, but not completely, reduced—that is to say, about half the body still remained unaltered. Again, when the pressure was fairly applied, collapse came on, which gave me great anxiety for a short time, the pulse being for some minutes nearly imperceptible. I then gave up the attempt, for the pulse had risen to 120 before giving chloroform, and the tongue had become dry.



There appeared little chance of doing further good, for I employed as much force as was consistent with safety. On both the occasions she was under chloroform. However she suffered no untoward symptom from the attempts at reduction; indeed, for some time after she had less than ordinary menses, and consequently improved much in general health. I heard about a year and a half afterwards that she was in fair health.

*Remarks.*—There can be little doubt that if the medical attendant had examined this patient when the nurse called his attention to “something being down,” the inversion would have been readily reduced. One cannot be too strict in practice in observing the rule in all cases, to fully go into all points; and this holds good particularly after delivery if anything be complained of, because nothing like a swelling can exist about the vulva without its being worthy of attention. If it be an inflamed labium, or an irritated pile, or prolapsus of the uterus, these all require attention, for they may lead to much misery, and, if neglected, will be sure to bring the medical attendant into discredit. But if it be a polypus or inverted uterus—particularly the latter—the importance of our attention is self-evident. I believe that most of our errors arise rather (now-a-days at least) not so much from ignorance as from a want of thoroughness in going into every point in a case. There is, especially after labour, a natural wish that everything should go on rightly, and there is consequently a dislike (which we all feel) to disturb this feeling. Anything being wrong half implies that we have not done our work skilfully, and we hope that all will come right in time. We shirk the inquiry which may rudely disturb our complacent feeling; but, in doing so, we but postpone the evil day. The existence of inversion is one of the last things we should wish to discover. To say that the womb is so disturbed from its natural arrangement that it will be necessary to place the patient under chloroform and perform some kind of operation, is a formidable thing to do, only ten or twelve days after labour; and yet it is better to do this than to allow the patient to go about with an inverted uterus, which is sure to give rise to symptoms requiring medical aid sooner or later.

In this case, the involution of the uterus to its natural size, and very probably the adhesion of the inverted peritoneal surfaces to one another, prevented its complete reduction, which, judging from its near accomplishment, we might fairly argue would have been easy shortly after labour.

The extreme condition of collapse is a point of importance, as it twice occurred at the critical attempt at reduction. In considering this and the next case, I am inclined to think that

this mode of reduction is not so suited for chronic inversion as for the acute form. It would be best, at any rate, to try the constant pressure with the elastic bags and the pressure recommended in the next case, first of all; and, if these failed, then to give an anæsthetic and try manual pressure. I would also suggest that sulphuric æther should be employed, either alone or in combination, as having a greater tendency to stimulate the heart's action.

At present, this case has not been followed by the menorrhagia and its consequent anæmia, usually found in chronic inversion of the uterus, although before treatment she was losing greatly. For some time afterwards, the quantity was much less than normal.

*Case 2* was that of a woman aged about thirty, in her third confinement. The child was delivered naturally, but the placenta required the introduction of the hand to remove it. The next day, there was retention of urine, which could only be evacuated, and that in small quantities, with very severe straining, extending over four days. At one time, no urine was passed for thirty hours. She did not, after the subsidence of this trouble, appear to have suffered; but about the tenth day she noticed that something came down to or through the vulva. Not much notice was taken of this, and she gradually got about; but, as she supposed that the womb fell, she merely put on an external support. However, at the end of three months she applied to me, without having consulted any one before about it. I found the uterus completely inverted, rather larger than the natural size, and slightly bleeding when examined.

She was taken into Guy's Hospital under my care, and, after a dose of aperient medicine, was placed under chloroform. I passed my hand into the vagina, and grasped the uterus to compress out the fluid within it. This reduced its size; and, by steady upward pressure, the inverted body passed nearly up to the level of the os. The other hand was placed externally to give support to the vagina. I was watching her condition at the time when the fullest pressure was being employed, lest any such collapse should occur as did in the last case, when I noticed her aspect staring and livid. It was then ascertained that respiratory movements had ceased. Artificial respiration was used, and galvanism employed down the course of the phrenic nerve and from the spine to the front. After a minute or two (the exact time was not taken) she showed signs of recovery, particularly on the use of the galvanic battery; and gradually her appearance became natural. We continued artificial respiration and galvanism five or six minutes, and then waited. She, however, shortly gave signs of relapse, after



which the same measures were continued, till, in about twenty minutes, she began to fairly recover. I did not attempt anything further for some hours, when two indiarubber ball pessaries were introduced into the vagina, and a pad placed externally, supported by a T-bandage. This was removed next day; and it was found that the cervical portion had become more relaxed. The same treatment was continued another day; and then, after the removal of the bags, an attempt was made, without chloroform, to complete the reduction. But, although the fundus went within the os, yet, in consequence of the pain produced and the recession of the parts, I considered it advisable to wait and to apply continuous pressure to the fundus. I therefore adapted a stethoscope, pressing the cup of the chest-end against it. The ear-end, of course, came outside the vulva; upon this a pad was placed, and the T-bandage firmly applied.

On removing the bandage, twenty-four hours afterwards, I found about two inches of the stethoscope within the uterus, firmly held by it; but by pressure with the finger on one side, and by tilting the instrument, it was removed. I found that the fundus had receded, and that the cup of the stethoscope had made its impression on the posterior wall of the uterus, giving somewhat the feeling, at first examination, that the fundus was not quite perfectly restored. However, further examination showed its true nature; and that the cavity of the uterus was forward and hollow upward, as naturally it should be. At the same time, I felt the want of an instrument which should have a convex end, instead of concave, so as to complete the reduction securely. This I have endeavoured to obtain by an easy adaptation to a stethoscope, which is described below.

A dose of ergot was given to promote the contraction of the organ, and the horizontal posture continued for about ten days. At the end of a week, the os and cervix were still somewhat open. An injection of alum-water was employed, and in a few days it was nearly natural, but with some degree of thickening and irregularity. She went out quite well. The parts contracted to nearly their original size.

The remarks made upon the last case are applicable to this. It is surprising that so little discomfort or shock was felt at the time, when we consider the formidable effects usually seen in inversion occurring suddenly immediately after labour. It is highly probable that the change took place slowly—that is, during three or four days. The treatment, which was unsuccessful in the last, was successful in this case; and there seem to be two points worthy of consideration. The first is, that the effect of the air-bags in the vagina is to dilate it, and along

with it the os and cervix. This is produced, probably, in part at least, by the stretching of the upper portion. This dilatation of the cervix enables the inverted body of the organ to pass into it; and then pressure of it in an upward direction stretches open the angle of flexion, and thus the way is paved for its slow restoration. As I have above remarked, attempts at sudden restitution are more likely to be unsuccessful; and I think it will be found best always to employ the air-bags for two days first, and then either to give chloroform and reduce, or to employ pressure directly to the fundus for a day. I am inclined to think, from consideration of the habits of the uterus, that the slower mode is the most certain.

I have had some bags constructed by Mr. Arnold, which I think will be more certain of producing the proper dilatation of the vagina and pressure on the fundus. The first to be introduced is a ring-shaped one, about four inches in external diameter, and its opening about two inches; the fundus uteri passing through the ring. This presses on the upper vagina equally all round, and steadies the uterus under the pressure of the second bag, which is a globular one of not too firm walls, and larger than likely to be wanted, about four inches in diameter, so that it may completely fill the whole vagina below the ring-shaped bag. This of course gives support to the first, and also presses directly on the fundus, thus preparing it for the more complete pressure to follow. A T-bandage, with pad externally, should then be applied. The apparatus for pressing on the fundus is a vulcanite stethoscope, having a pear-shaped elastic bag drawn over the thoracic end and tied round the stem tightly. Into the opening at the aural end a stopcock can be screwed, which is attached to the tube of the indiarubber spraying apparatus, by which means the bag at the other end can be distended.

To apply them to a case: The bowels having been previously opened, the ring-pessary, well greased, should be passed over the inverted uterus, and then inflated by its tube, which is then tied. The globular ball should then be introduced and inflated. A T-bandage, with a large perineal pad, must then be applied as tightly as can be borne. Twenty-four hours afterwards, these should be removed, and the vagina washed out with warm permanganate of potash lotion, or, at any rate, warm water. The bags are again introduced, and next day removed, with ablutions afterwards. The state of the uterus should then be examined—whether, indeed, it be not restored to its natural state. If it be not, then the thoracic end of the stethoscope should be pressed against the fundus as much as possible in coincidence with the axis of the organ—that is, with the stem pressed back to the posterior fourchette; a pad being placed on



the ear-piece outside it. A T-bandage secures constancy of the pressure. This arrangement is removed next day; and then, if the restoration have been accomplished, the end will have entered the cavity of the uterus. But, to secure the complete restoration of every portion, inflate the ball on the end by the apparatus described. If this be left for two or three hours or more, the air can be let out, and the instrument wholly withdrawn. The distension of the ball is intended to distend the uterus enough to allow the end to be drawn out before it can again close on it. It is well to give a dose of opium every eight or twelve hours, to lessen the irritability of the uterus. A simple stethoscope will do, as this case shows; but it appears to me that a more efficient instrument would be found in the one just described.

Should one day's pressure be not sufficient, it should be continued another day more; but in that case it will be well to allow an interval of half an hour to elapse before reapplying it. If that be unsuccessful, it would be well to give chloroform, and to employ manual pressure.

Excellent hints from various authors, on the restoration of the more obstinate cases of this kind, may be found in the *Obstetrical Transactions*.—*British Medical Journal*, Aug. 31, 1872, p. 237.

## 89.—RACHITIS AND MINERAL INANITION.

By Dr. BLACHE, Paris.

Let us first revert to what has been recently demonstrated in a work by M. Dusart, "*Archives Générales de Médecine et de Chirurgie*," 1869-70. This author has concluded from previous labours and his own experiments:—

1. That, in all living beings, whether vegetable or animal, the presence of phosphate of lime is necessary for the transformation into cells of the azotized matters supplied by food, and that, to preserve their vitality, the tissues must be constantly traversed by a kind of current of phosphate of lime.

2. That the vital activity and temperature peculiar to each animal species is always in proportion to the quantity of phosphate of lime contained in it.

3. That when the food is found to be deficient in phosphate of lime, the tissues draw from the osseous skeleton that which is necessary for the maintenance of the integrity of the functions of nutrition, as the organism is seen to borrow from the adipose tissue the hydrocarbon elements in which it is deficient in certain cases.

Let us now examine each of these points, and see what consequences may be drawn from them in the study of the subject

now before us. And, in the first place, by virtue of what action do albuminised substances assume, in presence of calcareous phosphate, the form of cells and of tissues of every nature, without, however, its being demonstrated that salt of lime forms any constituent part of them?

“Phosphate of lime,” says Lehmann, “appears to be a mechanical agent of nutrition.” But this term is very vague, and by no means dispels the obscurity that reigns in the mind of the reader. Knowing the property possessed by salts of lime of precipitating albumen in an insoluble state, whenever those two orders of substances are found in presence, might it not be admitted that there takes place in the organism a phenomenon analogous, with this modification—which is, however, quite natural—that in the living medium this precipitate assumes the figured form and becomes organized in tissues?

This interpretation will appear less hazardous if it is borne in mind that, in the muscles, the proteinous substances present themselves in two different physical states: the liquid form preserving the name of albumen, easily assimilable, soluble in water, and containing only alkaline salts; and the solid form under the name of fibres of different kinds, offering a greater resistance to the action of the digestive agents, and containing almost the whole of the calcareous salts.

From this it may easily be understood that if children too young or persons debilitated from different causes are subjected to a course of alimentation exclusively composed of the flesh of young animals (Guérin’s experiments), the liquid albumen will be easily absorbed, whilst the solid parts containing the salts of lime will resist the action of the debilitated stomach and will be ejected.

It is, therefore, not organizable elements which are wanting, but an organizing agent, and mineral inanition ensues indirectly as surely as by administering aliments deficient in salts of lime, such, for instance, as wheat alone.

We have said that the vital activity of animals, and their peculiar temperature, are always in proportion to the quantity of phosphate of lime they contain: now, in the physiological experiments that I followed out with M. Dusart, when animals were deprived of phosphate of lime, this privation was attended with a sudden loss of appetite, accompanied with sadness, depression and immobility, as infallibly as the contrary phenomena were produced, in the course of a few days, by administering calcareous salt.

The symptoms thus observed have the greatest affinity with those which mark incipient rachitis, and those periods of adolescence during which, after a rapid stage of growth, the organism, exhausted by the efforts it has just made, and inca-



pable of sustaining it any longer, seems to give way, and reveals a suspension of all the acts of nutrition, and a general atony by which the patient is affected morally as well as physically.

We have then, as in rachitis, pains penetrating the limbs and articulations, complete anorexy, fatigue and difficulty in breathing after the least exercise. This state also reminds us of that of a great number of women during their pregnancy, and of that of most convalescents when the agents of nutrition are slow in resuming their functions.

To explain the gravity of osseous injuries, we have said that the tissues subjected to mineral inanition draw from the skeleton as from a common reservoir the phosphate that is necessary to them. We justify this assertion by the fact, that in the animals on which we made experiments, we found, at the end of three months, that the bones had become friable and had lost two-thirds of their weight.

Now it is known that in a normal state the osseous tissue requires several years for its renewal; if, therefore, it had undergone only a mere stoppage of nutrition, ejecting the worn out elements and receiving no fresh ones, it would have lost but a slight part of its substance. Such is not the case, and we do not see that any other explanation could be given of this apparent anomaly.

But how is it that mineral inanition, whether artificially produced in animals or whether it is the consequence of different morbid causes, is manifested by lesions which vary in their characteristics?

A complete answer appears to us to be contained in the consideration of the age of the subjects attacked. If, in fact, vicious alimentation exerts its action on children too young, the invariable presence of ill-digested substances in the intestine will soon produce enteritis and all its consequences; whilst the bones will undergo a shock, the more destructive as they are struck in the midst of the intensest movement of organization. Then follow the thickening of the periosteum, with vascularisation, effusion of blood under the periosteum, and in the bony texture itself.

The latter, mortally injured and struck with a kind of molecular necrosis, is divested at once of its mineral and organic principles, and the phosphates subjected to the destructive influence are abundantly eliminated through the kidneys.

In vain are new tissues formed in the bone to replace those struck with inflammation; they remain in a spongoid or fibrous state till the progress of age or a judicious regimen admits of the introduction of fresh salts, susceptible of organization.

If, on the contrary, the morbid cause attacks subjects having

already attained a somewhat advanced degree of development, if the osseous system, having acquired its normal consistence, now presents only the movement of nutrition necessary for the regular renewal of its different parts, the perturbation will be less, and, though the osseous tissue will be singularly rarefied, we shall discover friability, but no inflammatory phenomena with mollities, except in very rare examples of osteomalacy, mostly occurring in very peculiar cases of late or multiplied pregnancies and under hereditary influence.

In conclusion, let us mention that from the very beginning of the pregnancy, the phosphate of lime disappears from the urines.

The salt is condensed throughout the organism with a view to the future development of the embryo, in the form of a thickening of the bones, and even of concretions, called by Follier osteophytes, which, far from being, as certain authors have believed, the result of an aberration of nutrition, ought, in our opinion, to be considered only as reserves destined to meet the extraordinary consumption caused by the rapid growth of the foetus towards the last months of the pregnancy. In fact, it is at that period that the osteophytes begin to diminish; to disappear completely only when they have supplied the milk, during the first months after the delivery, with the mineral elements with which it is abundantly provided at that period, and which are necessary for the sustenance of the infant.

Finally, if all these perturbations proceed from one and the same cause, solely arising from mineral inanition, to remove them it must only require the introduction into the organism, directly or indirectly, of the salts in which it is deficient.

We are of opinion that the success attending the use of cod-liver oil is owing to no other cause. Let us remark that, to produce such satisfactory results, it is sufficient to administer it only in feeble doses, fifteen grammes a day, for instance, the greater part of which does not pass through the digestive tube, and is to be found again in the stools without having been digested. It will be easily admitted that the oil does not act as food in such circumstances. We must then seek elsewhere the cause of its efficacy. As for us, we believe that it wholly proceeds from the exciting action produced through the whole extent of the digestive tube by the volatile oily acids to which it owes its well-known odour. Then reappear the regular contractions of the digestive tube, and especially the glandular secretions. This double influence promotes the digestion of the solid parts of food which had, till then, resisted, and carry along with them the salts of lime with which they are impregnated.

The same result is obtained, and our practice for several years



past is a sure guarantee of the fact, by presenting directly to the absorption of the stomach phosphate of lime; on the condition, however, that the latter be soluble and require of the economy no labour for its digestion and assimilation.

We lay great stress on this point, for before us, many physicians, without, however, suspecting the general action of phosphate of lime, had made a trial of it on patients with the sole view of obtaining a restoration of the bones; but the constant failure of these attempts had produced such discouragement that all had given it up, to such a degree that, if there are some who in a commonplace way advise the use of it, others formally reject it as useless, whilst a certain number, and among the most modern, never pronounce the name, even relatively to the treatment of fractures and of rachitis.

To discover the reason of this conduct, M. Dusart made on healthy dogs, provided with gastric fistulas, a series of observations on the digestion of the phosphates in use till then. He ascertained that, after long labour of the stomach, an infinitely small part was dissolved by the lactic acid, whilst the rest—that is to say, nearly the whole—passed without being digested, but only disaggregated into an impalpable powder in the intestine, where its most ordinary effect was to produce an obstinate constipation.

It may, therefore, be easily conceived that the phosphate taken into the weakened stomach of persons afflicted with rachitis will undergo no modification, and remain absolutely inert.

Then, imitating the action of the gastric juice on phosphates M. Dusart prepared a body to which he gave the name of lacto-phosphate of lime, containing the salt completely digested and, consequently, capable of being absorbed without requiring any previous labour of the stomach, as he easily ascertained with the dogs already mentioned.

It is this compound that was employed in the comparative experiments made on animals, and that we have made use of in our clinical researches.

This product is now known to most French physicians by the names of Dusart's Lacto-Phosphate of Lime Syrup and Wine.

The large dose of medicine contained in these preparations—a gramme of lacto-phosphate to each tablespoonful of syrup, and to each glass of wine—explains how, under its influence, patients are conscious, in a few days, of a return of appetite and of activity in all the functions of nutrition, forerunners of a permanent cure.—*Practitioner, Sept. 1872, p. 149.*

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## 90.—ON THE PHYSIOLOGICAL DYSPEPSIA FOR STARCHY FOOD IN INFANCY.

By Dr. PROSPERO SONSINO, Pisa, Editor of the Medical newspaper the "*Imparziale*," of Florence.

[There are certain conditions the absence of which, in the digestive system of early life, render it unfit to digest the aliments which are subsequently utilized. For instance, a young herbivorous animal lives upon its mother's milk, an animal product, and is unable to digest the vegetables upon which it will in after life be nourished.]

As the process of digestion consists essentially in the action exerted upon the aliments by different fluids which are poured into the alimentary canal; in order to obtain a good explanation of the question, it is necessary to inquire if there are any differences in the action of digestive juices in early life, compared with the adult age, and then to discover upon what materials of the aliments those differences act effectively.

If we examine the composition of milk, the proper food of the new-born and young infant, which with reason we may retain as the prototype of the complete aliments, we find in it, besides water and salts, the three organic groups of alimentary principles which concur in different manner to sustain the process of nutrition, viz., the group of albuminoid or *nitrogenized* principles—that of *fatty* principles; and that of hydrocarbonaceous principles. But it is remarkable that the latter are exclusively represented by a kind of sugar called *lactin*, which, with an insignificant modification, being converted into *glucose*, is rendered diffusible—a substance which could almost be called a true nutriment in the signification given to this word by Corvisart; whereas the starchy principle, which is a conspicuous ingredient of the diet of the adult, and which needs an elaborated process of digestion to be converted into glucose, the material fit for absorption and diffusion, is not at all present in *any* milk. This circumstance of the composition of milk induces us to put the question, "*Is the easiness of digestion of milk in early life to be attributed to its being deprived of starchy principle?*" or, in other words, "*Is it the inaptitude to digest the starchy matter which in early life interferes with the complete digestion of all the aliments which include that material?*"

If we have good reasons to reply affirmatively to this question, then it will follow that in infants there is what we may call a *physiological dyspepsia to starchy matters*, to which ought to be referred much, if not all, the difficulty in the choice of the convenient food for infants.

Let us see what is the principal difference of composition between animal food and vegetable food. Partly, that in the



latter we find more materials refractory to any digestion ; that in consequence a more elaborate power of digestion is needed to obtain the nutriment from it. We find in the one and in the other, albuminous principles, fatty principles, saccharaceous principles ; whereas *only starchy principles*, which constitute the principal ingredient of vegetable food, are not at all present in animal food, be it meat or any other tissue, or a product of the animal, as milk or eggs.

Therefore, the same physiological and anatomical arrangement that in infants gives evidence of its being not prepared for vegetable food, supports the conclusion that starchy matters cannot be well digested in infancy, and that there is really in infancy what may be called a *physiological dyspepsia for starchy aliments*.

Bidder and Schmidt found, on examining the power of saliva to convert starchy matter into glucose, that in some new-born animals that property is defective. The same physiologists collected some saliva of an infant four months and a half old, and found that this fluid converted the glue of starch into glucose with great difficulty and very slowly. Prof. Schiff, in his lectures on the physiology of digestion, says that the active principle of saliva appears in man only at the age of the first dentition, and confirms all the results obtained by Bidder, who was the first to make researches on this subject.

But saliva is not the only digestive fluid which assists in the digestion of starchy matter. Pancreatic juice and enteric juice also take part in it. As far as I know, no one has made any researches with the view of establishing whether or not these latter juices possess in new-born animals the same digestive power over starchy matter as in the adult animal. The performance of such researches on pancreatic and enteric juices is really more difficult than that concerning saliva, inasmuch as these juices are poured into the digestive canal, and we cannot therefore collect them in man as we can do with saliva. Thus the direct examination of the same juices can be instituted only in *animals*. But in the same manner as physiologists have established the general digestive property of pancreatic and enteric juices, especially by experiments on *animals*, it appeared to me feasible to elucidate the function of digestion in early life by analogous experiments on *very young animals*.

In making such researches I had the opportunity of availing myself of the advice and co-operation of my friend, the well-known physiologist, Prof. Schiff, who put at my disposal all the means offered by the laboratory in the Museum of Florence.

Prof. Schiff had long before instituted a large number of researches on the action of pancreatic juice in the digestion of adult animals. We had then to repeat them, as far as possible,

in the same manner on young and sucking animals, to give them a value for comparison between the digestion of the young and of the adult.

But to estimate the digestive action of pancreatic juice on starch in very young animals, we could not have recourse to the method of obtaining the pancreatic juice by a pancreatic fistula, as the smallness of the viscus in young animals renders the latter impracticable. We had, therefore, recourse to another expedient, which in the young, as in the adult, could be practicable with the same good result.

The process of experiment is the following:—After having killed the animal, the pancreas is immediately drawn forth, and being isolated, it is hashed and reduced to the consistence of pap. This is put in sufficient distilled water, and thus there is obtained a pancreatic infusion which is endowed with the same digestive powers as the pancreatic juice. If we pour some drops of this *fresh* infusion on some glue of starch, and afterwards apply to the mixture Trommer's test, we shall have evidence whether the transformation of starch into glucose has taken place, and therefore whether the pancreas of young animals possesses or not the digestive action on that material as it does in the adult.

We made the experiments with the pancreatic infusion of five sucking animals, *i.e.*, a little dog, five days old; another dog, fourteen days old; a cat, seven days old; and two rabbits, seven days old. The result was, that the fresh pancreatic emulsion of all these animals was incapable of transforming starch into glucose, *even after a long contact*, whereas the same *fresh* pancreatic emulsion of *adult animals* produces the transformation in a very short time indeed, *almost immediately*.

I think it very important to rigorously establish this condition of *physiological dyspepsia* in infants, which perhaps in the very young reaches absolutely the degree of *apepsia*, inasmuch as the tendency, not only of the public, but also among the generality of practitioners, is too favourable to feed infants with starchy matters. In fact, the preference given to starchy articles of food by mothers when they wish to add something else to the nutriment offered by the milk, or altogether to wean the infant, is notorious; and we find in many countries, at the present day as well as in the past, the common pap, or, what is still more dangerous, rice, arrowroot, or tapioca, which contain a larger quantity of starch than bread, united with the normal aliment of the sucking child; and, still worse, this is usually done more frequently, and with more persistence, when the infants do not thrive and when they are sick, *i.e.*, when the digestive power is probably more defective than in healthy infants.



The trials of administering food previously artificially digested both by gastric and pancreatic juices, in cases of infantile atrophy, are suggested in Routh's book already quoted. This author hints at the convenience of mixing with the food the pancreatic juice, particularly for converting starchy materials into glucose; but he adds: "Unfortunately, however, we have not yet accurate knowledge enough to prepare this artificial juice," And then adds: "It is to be hoped, however, the desideratum being known, that some means may be devised by which it may be procured, so as to admit of convenient employment in cases like those under consideration."

Now, there is no longer any difficulty in satisfying this desideratum. It is known, in fact, that the infusion of fresh pancreas acts like the pancreatic juice itself, and that if we obtain an infusion of pancreas in glycerine instead of water, we have a good manner of preserving it as long. Leube, who recently made some trials of feeding by the rectum, found that glycerine extract of pancreas is quite equal in its digestive power to the fresh pancreas, and will remain good for several weeks. The extract must be prepared with the pancreas of a bullock, which is finely chopped and rubbed with 250 grammes of glycerine. Thus, I think that we must try this new experiment to render digestible starchy matter in infancy.

But now a new question may arise about starchy aliments. Is the danger from them, in infancy, to be referred only to their indigestibility, or also to their affording insufficient materials for the sanguine process of assimilation, and to the feeble infantile process of hematosiis to reduce them conveniently for the ends of nutrition? If the second proposition is true, any expedient contrived with the view of enabling the alimentary canal to absorb starchy materials would be useless, the best thing being in that case to exclude them altogether from the food of infants. On this point it must be recollected that before the indigestibility of starch was hinted at, the danger of feeding the infant with it was attributed to its insufficiency for repairing the waste of the organism, and in this manner were explained all the disorders which arise from that manner of diet, to which was referred also the origin of rickets (Magendie among others).

Good reasons, however, make us now believe that really it is not convenient to feed infants with copious starchy matters, however these may be rendered digestible. These reasons are, that the nutriment furnished by starch does not afford materials for the re-integration of the principal tissues, but it concurs almost exclusively as fuel to the process of hematosiis, whereas the growing infantile organism needs a greater quantity of those nutriments which afford directly materials to the

development of the tissues than of the other, which must be consumed in the process of hematosiis. There is, moreover, reason to suspect that this function is in a certain degree defective in the infantile organism, and therefore that it is better accomplished with fatty matters than with starchy matters, the former being more advanced than the latter in their degree of oxidation.—*Practitioner*, Sept. 1872, p. 155.

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#### 91.—THE USE OF PANCREATIC EMULSION IN THE WASTING DISEASES OF CHILDREN.

By Dr. DOBELL, Senior Physician to the Royal Hospital for Diseases of the Chest.

[In 1871 Dr. Dobell intended to prepare an article for publication, "On the Use of Pancreatic Emulsion in Tabes Mesenterica." He however gave up the idea on account of the difficulty of proving in the cases which recovered that the mesenteric glands had been the seat of disease.]

In this paper I propose to drop the question of disease of the mesenteric glands, and simply to speak of the class of cases constituting that wretched form of "atrophy and debility" and "marasmus" in children, in which every part of the body wastes away except the abdomen; the state described by Dr. Druitt, in the last edition of his *Vade Mecum*, in the following few and graphic words:—"Emaciation and voracity; the belly swelled and hard; the skin dry and harsh; the eyes red; the tongue strawberry-coloured; the breath foul; the stools clay-coloured and offensive, sometimes costive, sometimes extremely relaxed; the patient usually dies hectic" (p. 75).

I wish to bring prominently forward the fact that this state, provided there is no advanced lung disease, is rapidly cured by pancreatic emulsion given in doses of a teaspoonful every four hours, and regularly persisted in till fat and flesh are restored. It is, of course, necessary that a proper diet should be insisted on at the same time; but proper diet without the pancreatic emulsion will not do. This I have found over and over again in cases where everything judicious in the way of feeding and cod-oil had been carefully and perseveringly tried without avail, but which, on the addition of the emulsion to the previous diet, began at once to improve.

This fact has been familiar to me for a long time; and considering how largely pancreatic emulsion is now used in the wasting diseases of adults, I am surprised to find that it is not even referred to in the latest works on the diseases of children. Looking through these works and examining their indexes, one is led to the conclusion that their authors are not aware that



there is such an organ as the pancreas, or that pancreatic juice has ever been used in any form in the treatment of disease. Yet scarcely a week now passes but some general practitioner relates to me cases of the successful use in his own practice of pancreatic emulsion in the wasting of delicate children; showing that in this respect the rank and file of our professional army are in advance of some of their generals, which ought not to be the case.

Dr. Prospero Sonsino's paper will, I hope, excite more general attention to this important subject. He, however, has laid all the stress of his observations upon the influence of the salivary and pancreatic juices on the digestion of *starch*. This is unquestionably a point of the greatest importance in the case of very young children brought up by hand, as showing the absurdity of attempting to nourish them upon starchy food, not artificially digested, before the period of life at which the saliva and pancreatic juice attain their functional activity. And even then, as Dr. Sonsino afterwards remarks, "good reasons make us now believe that really it is not proper to feed infants with copious starchy matters, however these may be rendered digestible." The principal results of Dr. Sonsino's investigations are summed up in the two following conclusions, which, however, are not new:—1. "Pancreatic juice in dogs, cats, and rabbits, in the first week of life—perhaps for some days more—is devoid of any digestive action on starch." 2. "In the early life of man, probably till the beginning of dentition, infants offer a true physiological dyspepsia for starchy aliments, caused by the inactivity of one at least—possibly of all—the humours that concur in the digestion of those aliments" (saliva, gastric juice, pancreatic juice, enteric juice).

No doubt, when wasting occurs in these early periods of life, it is very often due to foolish attempts to nourish children upon farinaceous foods, by which dyspepsia and diarrhoea add to the exhaustion of partial assimilative-starvation. But, as a matter of fact, farinaceous food is seldom depended upon without some addition of cow's milk or some assistance from lactation; and we see children suffer from wasting who are fed entirely upon cow's milk or nursed by their mothers, and in such cases the "physiological dyspepsia for starchy food" will not account for their decline. Therefore we must not forget, that although normal saliva only acts upon starch, normal pancreatic juice acts also upon fats; and it is probable that these two functions of the pancreas are sufficiently independent of each other that they may exist separately. This I pointed out in my paper to the Royal Society in 1868, "On the Special Action of the Pancreas on Fat and Starch" (Proc. Royal Soc. No. 97). It is there stated as the results of my experiments,

that "in addition to the influence of the pancreas upon fat, it has the power of converting starch into glucose by simple mixture. This property remains to a certain extent *after the pancreas has exhausted its property of acting upon fat*. The quantity of pancreas which before mixture with fat will convert about eight parts of starch into glucose, after saturation with fat will still convert about two parts of starch into glucose." It is possible, therefore, that in different states of depraved health one or other of these properties of the pancreatic juice—that for the digestion of starch or that for the digestion of fat—may be deficient. And thus the depraved nutrition due to such deficiency will not be limited to the period of life anterior to that at which, under normal conditions, the proper functions of the pancreas should be developed. It is evident that when the power of digesting fat fails to be developed at its proper time, the defect must tell with double force upon children already suffering from deficient digestion of starch.

The children who become the subjects of this kind of wasting of which I am now treating are especially (1) those who are sucked by mothers whose milk, though abundant in quantity, is extremely deficient in nutritive properties; (2) those who are brought up by hand; and (3) those who, at a later period of childhood, have been subjected to similar chronic defects in diet. Now, it is especially when the mother's milk is poor in fat and lactin that the child becomes "dissatisfied" and "craving," and in the majority of cases it is this which first leads to the introduction of farinaceous food, under the popular nursery belief that it is, "*satisfying*;" and, as Dr. Sonsino states, if this is given before the power of digesting starch is established, of course nothing but mischief can result.

But organs, like individuals, do not rise to the full performance of their duties unless called upon by the necessity for their activity; and, as I pointed out in 1866 (*On Tuberculosis*, p. 40, second edition), "As the mother is deprived of fat elements by lactation, so is the child deprived of them by a persistence in a diet deficient in milk. In the case of the child thus deprived of fat, a double injury is done—first by cutting off the supply of fat elements necessary for the protection of the tissues; and secondly, *by paralysing the function of the pancreas by prolonged inactivity*." I venture to think that this is a point deserving of far more attention than it has yet received. It accounts in a great measure for the impossibility of restoring these ill-nourished wasted children by any kind of *natural* diet after they have been allowed to remain in a chronic state of defective nutrition. A child that has been long fed upon diet deficient in fat fails to develop the fat-digesting properties of the pancreatic secretion, and thus, when



proper food is at last presented, cannot make use of it for nutrition.

It is probable, therefore, that it is due to this conjunction of circumstances that these wretched cases of fatal infantile wasting occur;—the food deficient in fat not only fails to nourish the child, but fails to develop the function of the pancreas for the digestion of fat at a later period of life; the craving of the child due to the deficiency of assimilated fat is met by starchy food which it has not the power to digest, and which if digested cannot supply the place of fat. Thus it is literally starved from first to last of those elements of nutrition especially essential in early life. We cannot, therefore, be surprised that such cases have proved obstinately fatal, neither is it anything but what one might expect, *à priori*, that they get rapidly well when pancreatic emulsion of fat is added to their diet, for by this means they are enabled to assimilate both fat and starch.

I have proved over and over again that, whether in children or adults, no amount of milk or cream, however good, will do instead of pancreatic emulsion; and I have tried to discover why this should be. Milk, so far as this part of its composition is concerned, is simply an emulsion of fat; and pancreatic emulsion, as I have shown, in the paper to the Royal Society already referred to, is not, as formerly supposed, a chemical combination, but a true emulsion. Why, then, does not milk answer as well? I believe the explanation to be very simple, and that it turns upon the following points:—

1. The fineness of the particles of fat.
2. The permanent character of the molecular mixture of fat and water.
3. The proportion of fats having high melting points.

(a) In my first paper on Pancreatic Emulsion, *Lancet*, (September 10, 1864), I gave the measurements (made by the late Mr. Farrants, president of the Microscopical Society) of the particles of fat in cod-oil and beef-fat emulsions, as then prepared for me; showing that the majority of the particles in the cod-oil emulsion ranged from the 16,000th, to the 1,200th of an inch in diameter, and these in the beef-fat emulsion from the 10,000th to the 2,500th of an inch; and, according to Bowman (*Practical Handbook of Medical Chemistry*, p. 174), “The size of the globules in healthy milk varies from a mere point to about the 2,000th of an inch.”

Since I published Mr. Farrants' measurements, pancreatic emulsion has been made by a much more equal and satisfactory process than at that time, and I have just examined a chance specimen procured from Messrs. Savory and Moore, in which the large majority of the particles of fat range from the

21,600th to 14,400th of an inch in diameter, the prevailing size being the 18,000 of an inch; while in a specimen of good new milk (cold), which I have also just examined, the large majority of the particles of fat range from the 7,200th to the 3,600th of an inch in diameter, the smallest being the 10,800th.

(b) The permanent character of the pancreatic emulsion is very remarkable, far exceeding that of milk. It “differs entirely from all other kinds of emulsion of fatty matter, whether chemical or mechanical. All other emulsions of fat are destroyed by ether, the fat being restored at once to its original condition. The influence exerted by the pancreas upon fats, therefore, appears to operate by breaking up the aggregation of the crystals of the fat. It alters the molecular condition of the fat, mingling it with water in such a way that even ether cannot separate the fat from the water. A *permanent emulsion* is thus formed ready to mix with a larger quantity of water whenever it may be added.” (Proceedings of the Royal Society, already referred to.)

(c) In the *Chemical News*, September 4, 1868, I have stated my reasons for believing in the importance of fats of high melting points, such as stearine, margarine, and palmatine, over those of low melting points, such as olein, as elements of food and medicine; although further experiments and investigations are still needed on this interesting subject.

Pancreatic emulsion of solid fat, consisting principally of stearine, margarine, and palmatine, is therefore quite a different thing from milk, the fat of which is principally olein.

Now, the nearest approach to a pancreatic emulsion is what may be called *nascent milk*, by which I mean milk just secreted—milk that flows from the mammary gland as it is formed, or, as mothers term it, “as the draught comes in.” In this the emulsification is finest and most perfect, but every minute that elapses after the milk is secreted deteriorates this perfection of emulsification, until, as we know, whether retained in the lactiferous ducts or in an artificial vessel, but especially in the latter, and when allowed to cool, the cream separates from the water of the milk, never again to be susceptible of the same emulsification with water in which it first existed, *except under the influence of pancreatic juice*.

I submit that this is the secret of the superiority of lactation, and especially of lactation at the time “the draught comes in,” over every other kind of infant-feeding, whether in man or in the lower animals. It forms an important distinction between milk-diet supplied by the natural process of suckling, and milk-diet administered artificially, and affords some reasonable colour to the old-standing belief in the efficacy of “new milk warm from the cow” for delicate children, and to the remarkable



recoveries recorded in ancient times of old persons nourished by lactation when everything else had failed.

The Author will be much obliged to any of his readers who will favour him with their clinical experience on the subject of this paper.—*Practitioner*, Oct., 1872, p. 234.

## 92.—ON INFANTILE PARALYSIS AND ITS RESULTING DEFORMITIES.

By RICHARD BARWELL, Esq., Surgeon to Charing-Cross Hospital.

[There is no doubt that infantile paralysis in its early stages is curable by the two forms of galvanism. Unfortunately, however, we frequently do not see the cases until too far advanced to be amenable to this treatment.]

Being strongly convinced of the peripheral origin of infantile paralysis properly so called, yet finding that in cases of some little standing no progress was effected by galvanism, neither was the internal use of strychnine followed by any benefit, I determined to employ the drug locally. For this purpose I injected into the moveless muscles or their neighbourhood the Pharmacopœial solution of strychnia. Here, too, I was disappointed; the topical effects were nil, even while on the verge of constitutional symptoms. It appeared to me that I must retard absorption, and thus, while keeping the drug longer in the part to be influenced, avoid its action on nerve centres. I therefore procured a very dense solution—viz., two per cent.,—and began a series of cautious experiments. I used a syringe with a piston graduated into half-minims, over which runs a screw stop; each turn and a half of the screw corresponds to a half-minim, hence each such quantity can easily be divided into six parts, and, with more care, into twelve; as each half-minim of solution contains  $\frac{1}{100}$  gr., subdivision is rendered easy and all precautions facile. In my experiments, however, I very soon discovered that, thus largely dissolved, strychnine might be safely injected in much larger quantities than had been imagined, and that I could fearlessly begin by giving even children five times the dose hitherto considered as the maximum—namely, five half-minims, or one-twentieth of a grain. But it was not alone with children that I thus worked: all local paralyses appeared to me proper subjects for these experiments. The results are singularly confirmatory of the idea which I had formed, that eccentric paralyses would be very amenable to the local action of strychnine. Thus, several cases of facial paralysis, several of paralysis of the wrist (independent of syphilis or of lead poisoning), and others, have yielded to a few injections, after months of galvanism, internal admin-

istration of strychnine, and other remedies have failed. Several of these cases have been published; here I will give only two short *résumés* in illustration of the power of the drug thus employed.

J. G. M——, aged thirty-eight, consulted me on the 14th January, 1872, on account of lameness from paralysis in both legs. In the early part of 1865 he had, at Malta, Mediterranean fever, which appears to be a variety of gastric fever. This was followed by acute rheumatism without heart affection. The recovery was slow, with many relapses. He does not know what muscles or limbs may have been paralysed when he was in bed; but when he began to get about, all the muscles in front of the tibiæ and all those in the ball of the thumb had lost power, except those inside the long flexor tendon, and even these were very much weakened. General wasting also of these muscles had taken place. His gait was very awkward and insecure; and he came to ask me about the desirability of instruments to assist him in walking, having found galvanism almost constantly employed utterly useless.

I explained the above-described use of strychnine, and proposed that it should at all events have a trial. He consented, and I began with the right leg, which was rather less wasted than the other. The dates and quantities of injection were as follows:—Jan. 14th, seven half-minims; 17th, eight; 19th, ten; 21st, twelve half-minims. From the time of the first injection the limb began to improve, both in power and in size. I then took the left leg—the more wasted of the two; but as the last injection had caused a slight sense of uneasiness I used a less quantity—namely, on Jan. 24th, eight half-minims; 26th, eight; 29th, eight; 31st, eight; Feb. 2nd, ten; 5th, ten half-minims.

In equal degree with the restoration to shape did voluntary power return. The limbs are not yet as strong as normal members, but this is a mere matter of time.

M. E. B——, aged forty, came under my care on the 17th of October, 1872, with paralysis of the right side of the face. On the 31st of August she, in washing her face, put her finger twice into the right eye. To this she attributes the paralysis; it was doubtless a consequence. In the evening her husband saw the “mouth go round.” She has been since the 1st September under treatment, taking tonics and strychnia, and using galvanism.

Oct. 17th. The right side of the face is perfectly immobile; the mouth much drawn to the left; the right eye wide open. I tested the irritability of the muscles with a rather sharp galvanic current. There was some action in the platysma, but nowhere else. I injected seven half-minims of the 2 per cent. solution of strychnia; no constitutional effects.



27th. This patient remained away ten days, as she was afraid of what she called "the operation." To-day, however, I found improvement, which she said had immediately followed the injection, but had not continued after the first three days; hence she returned, and I injected eight half-minims. On Oct. 29th I injected ten half-minims, on Nov. 1st, ten; Nov. 3rd, ten; on Nov. 5th, twelve; and on Nov. 7th, twelve.

I have noted here nothing beyond the mere injections, as steady improvement is the history of the case. After ten injections she was quite well, the mouth remained straight, and she could close the eyes perfectly. I saw her only a week or two ago, and she remained quite well.

It is to be observed that in this and in other cases of adult eccentric paralysis voluntary motion has been perfectly restored, while galvanic irritability has remained absent for a considerable time. In other cases a response to the continuous current has early arisen, but only after voluntary movement became markedly improved. The behaviour of infantile paralysis is herein different, and response to the electrical current from one or the other apparatus precedes any return to voluntary movement; but this also comes after a little while. My method of proceeding with infants is as follows: To begin by galvanising each muscle separately, and to note the amount (if any) of electric irritability, or its entire absence. The former organs may almost take care of themselves, a little galvanism being passed through them occasionally. The others should be galvanised two or three times, and closely watched to detect any sign of twitching. Now during all this time it is most essential not to frighten the child, or give more pain than necessary. It is impossible to make much out about the paralysed muscles of a limb while the part is being jerked from above by violent movements of the trunk. Although we do not expect electric mobility to come into the muscles which have long been paralysed, it is well thoroughly to test them. And now, having determined to use strychnia, I begin with from three to six half-minims, according to my patient's age; and, selecting an opponent to that muscle which is most contracted, I inject the solution into its substance, or sometimes over the place of entry of its nerve. I continue such injections two or three times a week for a certain period, and then try the muscles again. They nearly always respond first to the constant current, and usually soon after this a small amount of voluntary power becomes perceptible. The injections must not be discontinued as soon as the galvanic or nerve irritability is established; brisk response to the former should at least be secured.

L. C—, aged six years and six months, was paralysed in the left leg sixteen months ago, there having been neither

previously nor afterwards any severe illness. For the first ten months nothing was done except tonics and occasional rubbing. During the last six months galvanism and the internal use of strychnia have both been extensively used; indeed, on one occasion the drug was pushed so far as to produce tetanic spasms.

She came to me on the 6th June, 1870. The left leg was shrivelled and utterly useless. When she was supported by her nurse on the sound leg, the other hung impotently down. By a peculiar jerk of the body, common to all these children, she could make it swing; and the psoas being intact, or nearly so, enabled her to hitch it a little forward. The limb was always cold—blue in places, pink in others. The muscles were so wasted that the limb looked barely thicker than the bone should be. There was no response to even strong galvanic currents. Three minims of strychnine solution were injected.

I injected this child with strychnia twice a week for three months before I got any response to galvanism. When the response began I left off the injections and the irritability decreased markedly. I then, in October, recommenced the use of strychnia, combining it with galvanism, and this treatment was continued for five months. In March, 1871, I discontinued the injection. Considerable voluntary irritability of muscles had now been established, and very easy response to the continued current. In May, the induced current began to tell. The rest of the case is a mere question of time. The patient is still improving rapidly.

J. R., aged seven years and three months, was brought to me on February 3rd, 1871, with a deformity of the right foot, which I have named *pes cavus*, arising, as I have shown, from paralysis of the gastrocnemius, and frequently also of the muscles in front of the tibia. These paralyses were present in this case. The leg was a good deal wasted, but not livid. It easily lost temperature on cold days, but if kept well covered remained warm. The paralysis came on when she was two years old without illness of any sort; the deformity formed itself gradually. She has been treated, among other methods, with galvanism and internal doses of strychnine.

I injected five half-minims to begin with, and twice a week I continued this method, increasing to eight half-minims. At the end of seven weeks the muscles became amenable to the continued current. The use of galvanism was combined with the injection. At that time, viz., three lunar months—the muscles were voluntarily mobile, and the size of the leg was very much increased. The child walked very fairly well. The deformity was treated in a manner to be described in the sequel.



These two cases mark strongly a difference to be found in paralysed limbs—a difference which is of the very greatest value for prognostic purposes. In the one the extreme wasting and the great loss of temperature marked almost the utmost limit of nutritive disorder; in the other, although paralysis had lasted as long, or even longer, and though the limb was greatly wasted, yet the skin was not so cold nor so livid; the muscles gave, when pinched, some idea of resiliency, and were not like either soft paste in a sausage skin, or like bundles of cord, as in the former case. Now in these less livid cases, as it is certain that muscular degeneration has not gone so far, the chances of recovery within a moderate time are very considerable, however long the malady may have lasted. Of the other class I would speak with all caution. I have met with other cases besides that of L. C—— just quoted, which leads me to believe them curable, if sufficient care be employed for a long period. But we have to do not merely with malady. Three other conditions may, and do. often interfere—viz., convenience, patience, and money—to one or the other, sometimes to all of these, the best devised therapeutics will be forced to yield.—*Lancet*, July 6, 1872, p. 2.

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### 93.—A CASE OF SPINA BIFIDA CURED BY INJECTION.

By Dr. JAMES MORTON, Surgeon and Clinical Lecturer,  
Glasgow Royal Infirmary.

John Kelly, aged two months, was admitted into ward 17 of the Glasgow Royal Infirmary, on October 2nd, 1871. This child had a tumour opposite the upper lumbar vertebræ, globular in shape, of about the size of a small orange, and presenting the usual appearances of a case of spina bifida. Its covering was thin, so that it was quite translucent; and at the child's birth it was about half its present size. The mother stated that the tumour felt harder and fuller when the child cried. Moderate pressure upon it did not cause much inconvenience or suffering; and the child had never had convulsions, and seemed in all other respects quite healthy; the fontanelles being neither more open nor more full than usual at the same age.

On October 12th, the tumour was punctured with a grooved needle, and rather more than half of its contents—a pale straw-coloured fluid—was removed. It was then covered with lint dipped in oil, and over this cotton-wool; and, to fix the whole and afford a slight pressure, a broad elastic band was passed round the waist of the child. The sac speedily refilled, and on the 18th was punctured a second time; and again it refilled.

On October 24th, the tumour had regained nearly its former

size; and to-day it was punctured with a trocar and cannula of moderate size. After it had been half-emptied, a small portion—probably nearly half a drachm—of the following solution was injected:  $\mathcal{R}$ . Iodi gr. x; potass. iodidi 3 ss; glycerini 3i.

On October 26th, the child continued well, having no bad symptoms, and being only a little “fractious.” In the swelling, a portion of a soft solid substance could be felt, rather to one side.

On November 2nd, the mother was allowed to take the child home for a day or two; and on the 6th she returned to the Infirmary, when it was evident that some degree of refilling had taken place. The tumour was again injected with a small portion of the same solution. How much actually entered the sac, it was not possible to say. The child, however, continued quite as well as before; and, on November 23rd, he was brought up and shown to the clinical class, when the tumour was found to be smaller and harder than formerly, without any appearance of re-distension with fluid.

On December 7th, the child was again shown at the hospital; and now the swelling presented the appearance of a shrivelled bag of skin, darker in colour than the adjacent integument, somewhat resembling a corrugated scrotum, and affording reason to believe that there was now complete closure of the opening in the spinal membranes. The health of the child was perfect, his mother affirming that he never was so well and comfortable as he was now.

On January 8th, 1872, the child was again brought to the hospital, merely to show that he continued quite well.

*Remarks.*—In commenting on this case before the clinical class, a short account was given of the usual appearances presented by such tumours, and the ordinary coverings and contents were noted. Reference was also made to the deficiency in development of the osseous portion of the spinal canal, as the origin of the name, which of itself conveys no idea of the importance of the contents; and remarks were further made to the following effect. It is in reality a local dropsy, consisting of a collection of serum within the serous covering of the spinal cord; and, too often for the safety of the patient, there is also a portion of the nerve-structure of the cord, which has left its proper line, and lies extended under the serous lining of the sac, just under the skin, running, as it were, round or half round the circumference of the swelling, which is often globular, as in this instance. It is this condition chiefly which renders interference with such cases hazardous, and which presents to the mind of the surgeon the possibility of the nervous matter of the cord being so disturbed as to lead to very violent and dangerous manifestations of the effects of



irritation of such tissues, in the form of convulsions, which not seldom terminate fatally. Even when left to nature, the rule seems to be, that the subjects of such malformations die, and that early—within the first or second year. To this, it is well known, there are exceptions; still it is the rule. Attempts are usually made by relatives, under professional advice, to protect the part by the use of hollow cup-like shields, lined with cotton, and most carefully attended to; but the result is disappointment, so far as life is concerned. These tumours are far more common in the lumbo-sacral region than in all the other portions of the spine put together; and this is an example of one in the lumbar region.

*Treatment.*—Allusion has just been made to the palliative or protective treatment, as we may venture to call it; and we now turn our attention to the modes of attempting the radical cure, and giving shortly our reasons for adopting the plan which has been so fortunate in the present instance. Though it may be true that active surgical interference usually hastens death, yet cases have recovered after various kinds of treatment. The presence of the cord, and the free communication with the serous covering of the spine and brain, are the chief obstacles to success. Inflammation of the cord or its membranes, and gangrene of the cord, causing paralysis, may occur, speedily ending in death. Even when left untouched, the skin often ulcerates, the sac bursts, and palsy or convulsions cause death. A spontaneous cure has been known to take place, the orifice of communication having closed, the tumour becomes a closed cyst, and remaining innocuous, or it may be removed. Rupture of the sac has even been followed by recovery. Three modes of interference present themselves—injection, ligature, and excision; and of these three modes, it may be said that each of them has been effected by a variety of means and appliances, which is not my purpose to enumerate at present. Suffice it to say, that I resolve to adopt the first of these, as, in my opinion, the least dangerous of the three. It will be noticed that twice I punctured the sac with a grooved needle, and drew off a considerable quantity of the fluid. These may be called tentative measures, to ascertain whether the membranes could be pierced with safety; and, no suffering or derangement of function having followed, I felt encouraged to use an iodine solution. In forming this solution, I resolved to use glycerine as the solvent; for this reason, that it is a fluid of less diffusibility than a spirituous or even a watery solution. Not that I for a moment imagine that it could not be conveyed along the serous cavity or the serous lining of the spine; still, to my mind, it offered one element of greater security. The strength the solution may be noted; but then it must be observed that,

in using it, the sac was purposely only half-emptied of its serum when the iodine solution was injected; and of the latter only about half a drachm was actually injected, so that the then ioduretted contents of the sac presented a very dilute solution. When the repetition of the injection was made, somewhat more of the iodine fluid was used; but part of it escaped and was lost, so that, as the report states, it was not possible to estimate correctly the amount retained. A degree of solidification followed the first injection; and after the second it became complete, and now remains so, the part admitting of ordinary handling without inconvenience or discomfort to the child.

A line of treatment very similar to this has been followed by several of the American surgeons, and with instances of success; but statements are discordant in regard to the proportion which the successes bear to the failures. Several British surgeons have also operated successfully in a limited number of cases and by various methods, but most frequently by injection with iodine as the coagulating or stimulating agent; but I am not aware of glycerine having been used as the solvent on any previous occasion, and I may indulge the hope that others may be induced to try it.

From what I have seen of such cases, it is my belief that the fluid should not be allowed to be drained completely from the sac: this leads to fatal results; and, where a puncture has been made, the aperture, however minute, may be closed and guarded by a layer or layers of collodion, especially when subsequent oozing is feared or perceived.

[A second successful case is related in the same journal, for June 15th, by Dr. Morton.]—*British Medical Journal*, April 6, 1872, p. 364.

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#### 94.—NEW SPONGE-TENTS, &c.

These tents are made on a new principle by Salt and Son, Birmingham, and have the following advantages over those generally in use, inasmuch as they can be sold at 6s. per dozen, and they dilate more rapidly than the tallow-covered tents. These new tents are coated with cocoa-butter, and therefore, do not become displaced before they begin to expand. They are easily introduced, and, unlike the uncoated tents of Dr. Marion Sims, do not become spoiled if not dexterously inserted. They are carbolised, and are made of the actual shape of the "cervix"—oval. Cakes of the solid compressed sponge may be had, from which the surgeon may cut a tent of any size, from those fit to use in the urethra to such as are used for the uterus or rectum. These tents have been made according to directions given by Mr. Lawson Tait.—*Med. Times and Gazette*, Nov. 23, 1872, p. 586.



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